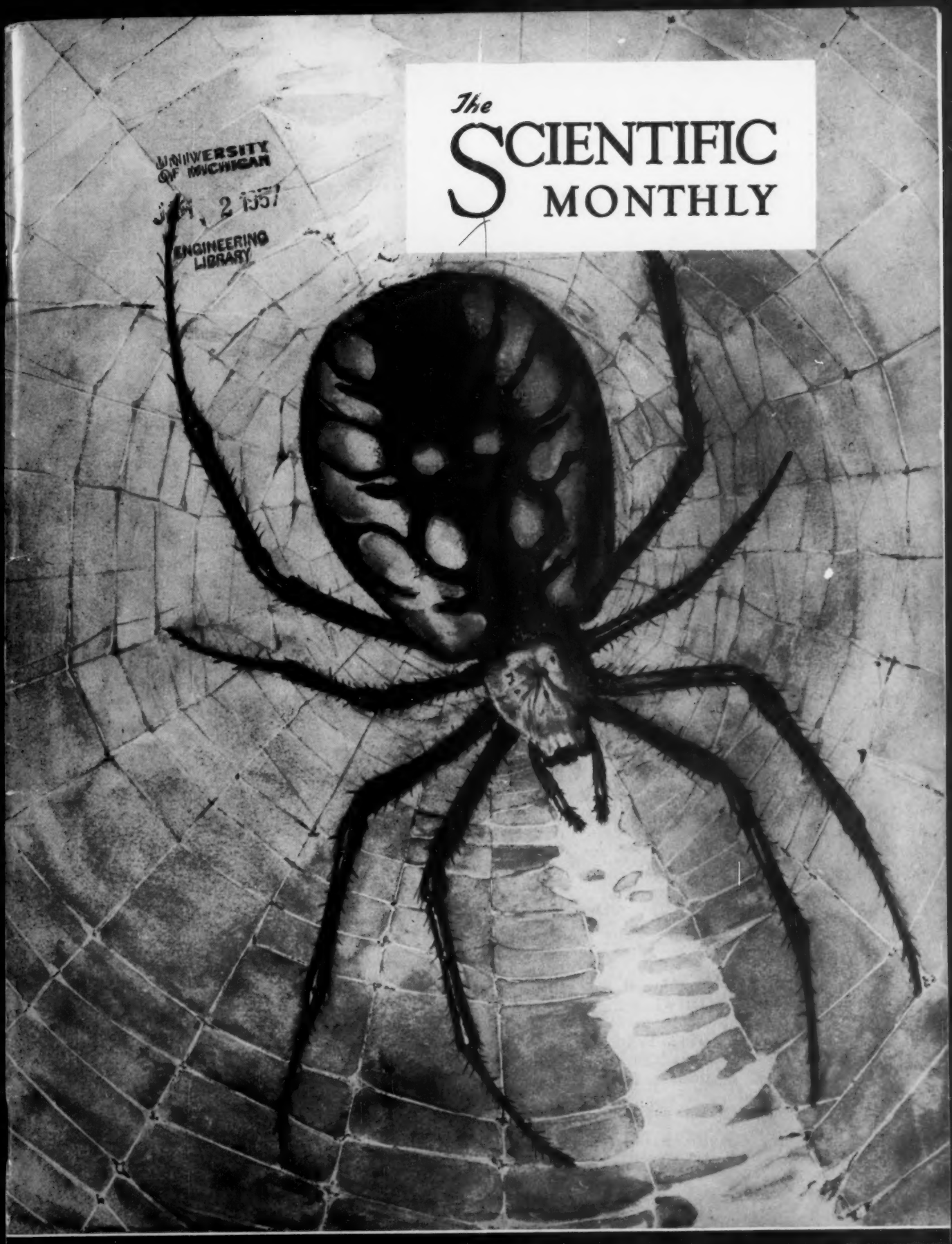

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(Left to right) Dr. John Bardeen*, Dr. William Shockley* and Dr. Walter H. Brattain, shown at Bell Telephone Laboratories in 1948 with apparatus used in the early investigations which led to the invention of the transistor.

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Drs. John Bardeen, Walter H. Brattain and William Shockley are honored for accomplishments at the Laboratories

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**Dr. Bardeen is now with the University of Illinois, and Dr. Shockley is with the Shockley Semiconductor Laboratory of Beckman Instruments, Inc., Calif.*

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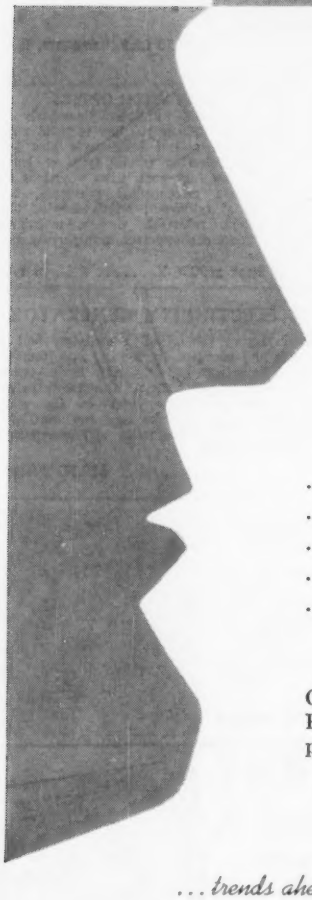

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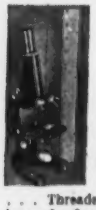

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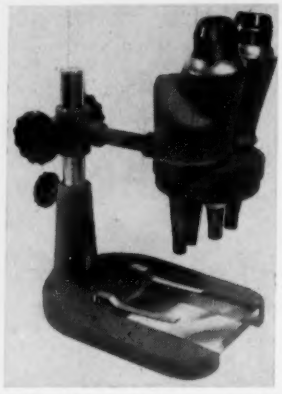

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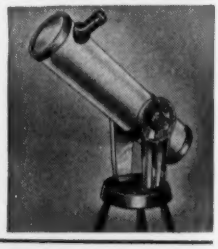

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Diffraction Information

Discussed in a new brochure on x-ray diffraction are basic theory (Bragg's law), definitions, analytic advantages, and applications in metallurgy, chemistry, mineralogy, physiology, pathology, and biology of x-ray diffraction techniques. Included are typical patterns of Laue, rotating crystal, and powder methods of crystal study. (General Electric Co., Dept. M41)

Fluoro-Silicone Rubber

A heat-resistant rubber has been developed that withstands swelling by fuels and oils. Hardness, tensile strength, and elongation are comparable to other silicone rubbers. (Dow Corning Corp., Dept. M42)

Mixer

A new pug mill mixer for laboratory use has a variable-speed drive of 20 to 30 rev/min. The instrument is driven by a 0.75-hp motor. (Lindcraft Corp., Dept. M45)

Multienvironment Chamber

A variable-temperature test chamber maintains temperatures ranging from $-100°$ to $+400°$F to within ± 2 Fahrenheit degrees accuracy. The stainless-steel chamber has a capacity of 12 ft^3. (Mantec, Inc., Dept. M46)

Random-Number Generator

A device called a stochastic generator has been developed which is capable of continuous production of random numbers at 10 per second and may be used as a computer accessory. (Loyola Laboratories, Dept. M47)

Oxygen Analyzer

A new portable oxygen analyzer, model D2, may be used for measuring oxygen in almost any mixture of gases. It is designed for use in such applications as measuring excess air to improve combustion efficiency, monitoring leakage of air in inert systems, and insuring safe entry into sewers, mines, and vessels. The accuracy of the reading is not affected by other gases in the mixture. (Arnold O. Beckman, Inc., Dept. M54)

Separatory Apparatus

Using three-phase solvent-system combinations, a new counter-current apparatus provides for automatic or manual shaking. (Laboratory Glass and Instrument Corp., Dept. M48)

Flask Supports

Neoprene rings 5 in. in diameter are designed to support hot round-bottom flasks. The rings are beveled to hold either a 250- or a 500-ml flask, and they do not slip on wet or dry bench tops. During storage, a ridge on the bottom of each ring interlocks with the top of the ring under it. (Fisher Scientific Co., Dept. M55)

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References and Notes

1. This article is based on a paper given in the symposium, *Species That Feed Mankind,* AAAS meeting, Boston, Mass., 27 Dec. 1953. The studies described were supported by a grant from ..... Acknowledgment is due to John Doe and Helen Blank for assistance in conducting the experiments.
2. W. C. Allee, *Science* 97, 517 (1943).
3. The two sets of observations represent .....
4. A. K. Goodwon, *An Introduction to Ethics* (Myer Press, New York, ed. 2, 1949), p. 124.
5. This dimensional theory is more fully described in .....

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THE
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JANUARY 1957

Uses and Effects of Atomic Radiation

In connection with the dedication of the new headquarters building for the American Association for the Advancement of Science, an all-day scientific program on the uses and effects of atomic radiation was held on 12 October 1956 at the Carnegie Institution of Washington, followed by the dedication ceremonies and an open house at the new AAAS building. Four of the symposium papers, by Shields Warren, L. C. Dunn, Lawrence R. Hafstad, and Laurence H. Snyder, and the dedication address, by Paul B. Sears, are printed here.

Radiation and the Human Body

SHIELDS WARREN

Dr. Warren is a member of the staff of the Cancer Research Institute at New England Deaconess Hospital, Boston, Mass.

THREE times man has met challenges beyond his power of physiologic adaptation and new to his evolutionary experience. The first of these was means of survival in ocean depths. The second was the problem of survival in the bleak, thin air of super-Himalayan heights. These two he has solved by providing small compartments of favorable environment where machines can supplement his own shortcomings and within which he can function along established and familiar patterns. The third new challenge that his environment offers has yet to be adequately met. This is the man-made environmental challenge of increased levels of ionizing radiation.

Detection

Heat, cold, hunger, cut, and blow are all clear to us as dangers to be avoided, and they are dangers to which our senses alert us. Radiation, on the other hand, is not detected by our senses, and we have to depend on specially devised techniques to recognize its presence and alert us to means of avoiding its insidious ill effects. Fortunately, these techniques are readily capable of detecting very low levels of radiation, far lower than those having any biologic significance. It is the ability to measure very low levels that has made possible the development of safe and valuable methods of diagnosis and research with radioisotopes.

The photographic emulsion either in the highly specialized form available for autoradiography at the microscopic level, where radioactivity on the order of a small fraction of a microcurie can be detected, or in its more common form—the radiation film badge—where amounts of radiation ranging from 0.05 to 7 roentgens may be determined under ordinary working conditions, is a convenient means of radiation detection and rough quantitation. With specially selected emulsions and known energy

of radiation, a range from 0.1 to 40,000 roentgens may be covered (*1*). Because of its value as a permanent record, ease of handling, and low cost, the photographic film has probably become the most widely used of all means of detection of radiation. The scintillation counter, the Geiger-Müller counter, and the ionization chamber in its various forms have significant value for selected determinations. As a result of the refinements of technique, it has been possible to determine with great accuracy the background levels of radiation and the total radioactivity of man, animals, and plants on the one hand, and very high dose levels, as from a supervoltage x-ray apparatus, on the other hand. If one combines the techniques of radiochemistry with these physical means of measurement, extremely low levels of radioactivity can be determined, as, for example, in studies on the strontium-90 content of human bones or of foods rich in calcium.

Biologic Effects

Unlike fire, which produces at once pain, blister, or char, exposure to radiation may antedate by weeks or even years its biologic effects. Some degree of repair follows radiation damage to somatic cells. Thus, a dose of radiation that would be lethal if given at one time will produce little change in body tissues if it is spread over a protracted period, but the genetic effect appears to be independent of rate. The lessened effect with protraction of the dose is of much use in the clinical treatment of certain cancers, for normal tissues can often repair themselves to a greater extent than the cancerous tissues.

The natural background radiation, which is contributed by traces of radium and related elements, our own intrinsic radioactive potassium isotope, and by cosmic rays, has been energetically supplemented by man in recent years with medical, industrial, and experimental sources of various types of radiation, including nuclear weapons testing. In those countries that are most advanced medically, the largest single contribution to the radiation received by man has been the medical use of x-ray radiation, particularly in the diagnostic field. Estimates in different localities indicate that the amount of radiation received from diagnostic x-rays is on the average about 3 roentgens to the gonads in a 30-year period, as contrasted to irradiation of germinal tissue from the natural background sources of about 4 to 5 roentgens in the same period (*2*).

A number of studies indicate that the development of leukemia in man seems to be one of the more delicate means of determining exposure to radiation. While some individuals can receive relatively large amounts of radiation without developing leukemia, in others radiation seems to be an effective causative or triggering mechanism. There appears to be statistical evidence for an increase of leukemia, apparently without a clear-cut threshold level for its induction. The lowest incidence appears to be the report of Stewart (*3*) that, in the case of children with leukemia, twice as many had been irradiated *in utero* as those whose mothers had not had uterine diagnostic radiation during pregnancy. This evidence suggests that as little as 2.5 to 12 roentgens delivered to the developing embryo may show a statistical detectable increase in the rate of leukemia in the child. The greater prevalence of leukemia in radiologists has long been known (*4*); likewise, the increased incidence of leukemia in survivors of the atomic bomb explosions in Japan has been clearly established (*5*).

With the dawn of the atomic age, it has become necessary as never before to understand the biologic effects of radiation and to balance potential good against potential harm. Never before has the development of a new field of effort been carried out with as much care for the health and safety factors involved. A considerable body of knowledge has been available on which to draw through the experience of radiologists, physicists, and others concerned with these sources of energy. We do not yet have all the answers, but a number are available.

Changes brought about in the germ plasm are in general harmful and are cumulative, for there is very little repair of damage. In general, the only repair seen in germinal cells is that brought about by recombination of chromosome breaks. While, in some plants and animals where rigorous selection can be carried out, increased mutability is of benefit, most mutations encountered in man probably must be considered as disadvantageous to the race. In addition, Muller (*6*) and others have educed evidence that the presence of recessive genes in an organism, even though they do not produce significant change in a particular characteristic, nonetheless may have a deleterious influence on the entire organism. That the deleterious effect of a recessive gene may be quantitated has been indicated by the work of Knox (*7*) in his study of the enzyme responsible for oxidation of phenylalanine, which is of importance in oligophrenic pyruvia. The portion of enzyme that stems from the abnormal gene in the heterozygote is defective.

Total body radiation is much more harmful than radiation of a localized part, as, for example, in the treatment of a skin cancer. In the former instance, one-tenth or less of the dose used in the latter instance may be fatal.

The several body tissues vary greatly in their re-

sponse to radiation. Lymphoid, blood-forming, and germinal cells are very sensitive, showing acute evidence of damage after irradiation with 100 roentgens or less. Striated muscle is one of the more resistant tissues, withstanding several thousand roentgens (*8*). The clinical responses vary with the tissues involved and with the dose.

Except in large doses, the changes induced by radiation may be long delayed, appearing months or years after either acute or chronic exposure.

Excessive radiation may cause the death of tissues, blood disorders, or cancers. There is much evidence that overdoses of radiation lead to premature aging. Both animal experiments and observation of the life spans of radiologists indicate that a dose of 1000 roentgens received over a long period of time may well shorten the life span about 10 percent. Data on the longevity of more than 82,000 physicians indicate that the average length of life of those not known to have had contact with radiation in the period of 1930 through 1954 was 65.7 years as against an average life span of 60.5 years for the radiologists (*9*). Not only is leukemia more prevalent among those exposed, but death from causes such as heart disease and arteriosclerosis also appears to come at an earlier age. In fact, radiologists succumbed at an earlier average age to practically every type of disease, indicating that the damage done to the body is widespread in its influence.

The level of radiation required to double the mutation rate has been variously estimated at from 5 to 50 roentgens. The National Academy of Sciences committee chose 10 roengtens as a conservative figure, and since more than half our children are born to parents less than 30 years of age, a dose of 10 roentgens delivered to the gonads up to the age of 30 is considered acceptable.

To provide safety for those who work with radiation, permissible dose levels have been evolved, one for occupationally exposed workers where healthy adults are carefully checked for health and radiation safety, and one for large populations where persons of all ages and in all states of health are involved. The permissible level for the former is 5 roentgens per year at a rate 0.3 roentgen per week; for the latter, the level is 1 roentgen per year.

Moreover, the occupationally involved group is as yet too small to have appreciable genetic effect on the total population. Since the permissible levels are set primarily for genetic reasons and are lower than those required to protect against somatic damage, the present levels may be regarded as giving satisfactory protection.

More than 250,000 measurements of radioactive fallout made in many localities here and abroad indicate that tests of atomic devices may be safely continued at the present rate for years. Because of its long half-life and its chemical and metabolic similarity to calcium, strontium-90 has been of particular interest in fallout (*10*). The levels of radioactive strontium at present attained from fallout or industrial atomic wastes have not reached disturbing levels in air, water or soil. Likewise, the levels of strontium-90 in foodstuffs (and particular attention is paid to those foodstuffs that are important sources of calcium for the diet) and in animal and human bones have not reached levels that are capable of producing pathologic effects. Careful and continuing study must be given to control this substance which may, if it is deposited in significantly increased amounts, be a source of danger.

X-rays

It must be remembered, in connection with radiation hazards, that x-rays often provide life-saving information and are necessary in many instances. However, there has been a tendency to use them too freely; this tendency is now being better controlled. Particularly to be avoided are unnecessary x-rays, such as those of infants taken merely to demonstrate their normality and x-rays of pregnant women when other methods of pelvimetry carry much less potential harm.

Since, even with care, greatly increased numbers of atomic plants will bring added problems of disposal of atomic wastes, particularly the troublesome bone-seeking isotopes, such as strontium-90, we cannot regard the health problems of the atomic age as solved even though they are not acute now.

Research is urgently needed in methods of waste disposal, in ways of protection from radiation, in cure of radiation injury, in basic radiobiology, and in genetics. Pending the finding of answers from these fields of research, we can comfort ourselves that our present probings into the atomic future can be safely continued and we can continue to obtain much essential help from the use of medical x-rays. However, we must not be reckless in exposing ourselves to radiation, and it is well to paraphrase the travel slogan of the last war and ask ourselves, "Is this x-ray really necessary?"

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Radiation and Genetics

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THE problem we have to consider is the relationship between a permanent change in our environment—an increase in the amount of radiation to which man and the animals and plants on which he depends are being subjected—and the effects that this change may have on the course of evolution of all the organisms concerned. The historical changes in plant and animal species—evolution—result from alterations in the balance between forces inherent in their biological systems, chief of which is the mechanism of heredity providing for continuity, and influences from the environment in which they live and to which they must always maintain an adjustment. Neither of these can ever be expected to be constant.

The system of genes by which continuity of life is maintained is subject to change by random alteration in some of the thousands of elements of which it is composed. This is known as *mutation* and may be expressed by the probability that a gene, during the process of self-duplication which occurs at each cell division during growth and reproduction, will give rise to an imperfect copy of itself and thus have a different effect in some descendants. Under normal conditions, this probability is quite low, so that hereditary characters maintain themselves in populations. The hereditary constitutions of populations are subject to changes at some slow rate that is dependent basically on the rate of mutation. This rate is subject to increase by several agencies, the most important of which is radiation.

A corollary of mutation and of inheritance transmitted by genes (Mendelian heredity) is that a variety of the forms of any gene, and the large number of combinations of these which arise under bisexual reproduction, will be retained in a population unless they are positively detrimental under the environmental conditions in which the possessors live. The environment with which the hereditary constitution of each organism continuously interacts is also subject to a variety of changes from natural (and now from man-made) causes. These may have two effects pertinent to the present discussion. One that is chiefly concerned with the level of radioactivity is alteration of the rate of mutation; the other makes the environment more or less hospitable to some of the hereditary constitutions of which the species is composed. The interplay between the variable environment and the variety of hereditary constitutions is known as *natural selection*, and this is the chief agency of evolution, the chief means by which historical changes in the heredity of the population occur.

The scientific problem can now be stated somewhat more precisely. What evolutionary effects may be expected from an environmental factor that acts directly to change the probability of mutation? That this is the effect of increased radiation on heredity is agreed by all authorities. It constitutes a new kind of problem. Previous discussions of the influence of changing environments on evolution have usually dealt with indirect effects acting through natural or artificial selection on hereditary constitutions composed of genes that were subjected only to natural or spontaneous mutation; now we have to deal with changes in the mutation rate itself.

The assessment and evaluation of the facts about the relationship between radiation and changes in heredity are difficult enough, especially when applications to human populations are in question, for we have little direct evidence on human mutation rates and the effects of radiation upon them. There is, however, a further problem that must be faced if our present views on this question are to serve as a basis of public policy. This is the problem that accompanies all political applications of science, but we confront it here in clear and acute form, for radiation risks affect everyone, everywhere, and as we shall see, they are cumulative in time, and our knowledge about their long-term effects is incomplete and uncertain. In dealing with this as a pub-

lic question, we may expect to find revealed divergencies in the ways of thinking that have become characteristic of scientific investigation on the one hand and of policy making on the other; and difficulties will certainly arise from the inertia that is inevitably associated with habitual ways of thinking and behaving.

In giving advice, a physician must have the buttress of confidence in order that confidence may be engendered in his patient. Political leaders responsible for policy must convince others that their views rest on reliable reasons and must convince themselves that this is so before they convince others. The goal must often be persuasion. In research scientists, on the other hand, a habit of mind has been acquired which accepts evidence only in the form of working hypotheses to be tested by further observations. Applications, by preference, will be delayed pending the building up of higher degrees of probability of correctness, and the scientist will usually distrust and resist persuasion by other means.

These two sets of occupational attitudes—the one which cannot avoid action and thus must have its decisions supported, the other which requires conviction before action is initiated—will often be in fundamental opposition. In the ordinary course of public applications of science, these conflicts come to resolution in practical and rather haphazard ways. In the making of policy concerning radiation hazards, which is perhaps the most important public question of our time, the resolution may be facilitated by elucidation of the sources of conflict and uncertainty, both with respect to the scientific facts themselves and with respect to the methods used in obtaining and judging them for public use. The attempt to resolve this dichotomy may be one of the chief instruments of progress as we move further into an age that is bound to become increasingly dependent on science and on the wide diffusion of scientific knowledge and understanding.

Radiation and Mutation

It has been known for 30 years that high-energy radiations (such as those from x-rays) increase the number of new mutations in the gametes (egg and sperm cells) of animals and plants. This proof was made possible by the prior elucidation of the mechanism of heredity upon which the radiation acts. This mechanism consists of an organization of thousands of living elements of a molecular order of size, the genes, arranged in a set of larger structures, the chromosomes, which occur, in numbers characteristic of each species, in each cell of the body and in each sex cell or gamete. In man, for example, each body cell contains 24 pairs of chromosomes, and each egg and sperm contains one member of each of these pairs.

Each chromosome is made up of hundreds or thousands of gene elements arranged in linear order. Each gene occurs in one of several alternative states, known as alleles, and so when each gamete is formed there passes into it a set of alleles that are transmitted to the next generation. Alleles are subject to transformation of form leading to differences in effects on the characters of the organism by a slow secular process known as mutation. Thus an allele *A* runs a risk of changing to a form *a* with a probability of the order of 1/100,000 to 1/1,000,000. When two alleles of form *a* are transmitted to a newly formed embryo, one from each parent, a character, such as albinism, may be observed in the offspring, differing from the normal character of the *Aa* parents. Such a character, requiring a like allele from each parent, is referred to as a recessive. Sometimes the recessive allele *a* changes by mutation to the dominant form *A*; and sometimes the structure of the chromosome or the arrangement of alleles in it changes by mutation and is transmitted to the descendants.

What was proved by Muller for *Drosophila* and by Stadler for plants, now some 30 years ago, was that the application of x-rays or radium (later extended to other forms of ionizing radiation) speeds up the process of mutation by increasing the probability of change of one allele to another or of grosser changes in the chromosomes. The increase in the rate of mutation is roughly proportional to the amount of radiation to which the reproductive cells are exposed.

The normal mechanism of heredity had to be known before such proof could be given, because most mutations are recessive and do not come to light until after several generations, when the same mutated allele, by appropriate matings, is contributed to the offspring by each parent. It was also necessary to distinguish between new recessive mutations, brought about by radiation, and old ones that had occurred previously and had been carried hidden in the ancestors until they were revealed by matings of two parents each hiding the same allele. Essential too was a knowledge of normal rates of mutation in the species without artificial radiation. All these requirements were successfully met for many species of animals, plants, and microorganisms, in all of which radiation was shown to be effective in increasing the mutation rate. It is only recently that such proof has been provided for mammals, and then only for one species, the house mouse. The pertinent evidence

has been reviewed for laymen by Charlotte Auerbach in *Genetics in the Atomic Age* (*1*).

At the time of discovery of new sources of radiation from atomic fission, a general theory of radiation genetics was available as well as a good deal of information on certain experimental organisms. This material has been reviewed by specialists in the three volumes on *Radiation Biology* edited by Alexander Hollaender (*2*). Marked gaps in both fact and theory were apparent, the chief of which were (i) that there were no secure data by which risks from radiation could be extrapolated from lower forms to mammals and man; (ii) that the fate of induced mutations in the population had not been worked out; and (iii) on the theoretical side, that no proof had been given of the actual physical mechanism by which mutations were induced. The original "hit theory," which assumed the essential event in mutation to be a direct encounter between a gene and the path of an energetic particle, was shown to be inadequate to explain all the facts. The result was that neither the basic knowledge of causal mechanisms nor empirical experience with mammalian or human genes was able to provide the basis on which protection against genetic damage from atomic radiation could rest. This gave rise to uncertainty and apprehension concerning risks involved in the weapons-testing program and in the increased uses of radiation in medicine and industry.

Study Commissions

It was against this background that study commissions were appointed by the National Academy of Sciences (U.S.) and the British Medical Research Council to prepare the reports on radiation hazards which have now been issued. In each report, the longest section deals with genetic effects, making these the most extensive organized efforts yet made to acquaint the public with the present state of scientific opinion on the effects of atomic radiations on future generations (*3*).

The American report, in fact, is more intent on educating the public than on documenting the evidence (which will be done in a later report). In the British report the reverse is true. It is perhaps a sign of differing attitudes in the two countries that the American report appears to give greater weight to the dangers and states them with fewer qualifications; the British report states the same facts with less emphasis. Neither is alarmist, although both regard radiation hazards with seriousness.

These reports, recognizing the paucity of data, have a tentative and preliminary character, yet encourage the belief that we are at least on our way toward necessary knowledge and awareness. They reach, with respect to the genetic effects of radiation, results that are essentially similar. What is especially encouraging is the assurance they give that all concerned, statesmen and scientists alike, recognize the necessity of doing the best we can with the knowledge we have. Although the chief recommendations in both reports are for more research, it is clear that this is not intended to delay the utilization of what we know now.

Each report assumed that the primary problem was to assess the hazards, for future generations, of increases in the mutation rate to which human populations may be subjected by increased exposure to radiation from a variety of sources, medical uses in diagnosis and therapy, atomic energy power sources, and the use of atomic weapons in testing and in actual war. There are two assumptions implicit in each report: (i) that induced mutations are qualitatively the same as spontaneous ones and, thus, merely add quantitatively to the total number of mutations in the population; and (ii) that the effects of a mutation on fitness are qualitatively similar in the homozygous and in the heterozygous condition—that is, when the offspring receives the same recessive allele from each parent. As I indicate in a subsequent paragraph, both of these assumptions are open to serious question. In fact, the testing of these two assumptions constitutes two of the essential problems in the field of radiation genetics, although this is not recognized in either report.

Both reports agree that the effect of radiation on mutation is cumulative throughout the reproductive period—a little today and a little tomorrow always adds up to increased risk of mutation. Both agree that new mutations nearly always have deleterious effects when they are homozygous, and that this is because newly occurring mutations have not been exposed to the action of natural selection, by which deleterious mutations arising in the past have either been eliminated, or else the genetic constitution of the population has adjusted itself to render them less harmful. The reports agree also that there is a "normal" or background rate of mutation inherent in the nature of organisms and of their environments and that included in the latter is a background of inescapable radiation from radioactive elements, cosmic rays, light, heat, and other natural sources. This background rate of mutation is referred to as the normal or spontaneous rate to which the genetic structure of each species, including our own, has adjusted itself. All three of these assumptions (cumulative effects of radiations on mutation rate, deleterious effects of new muta-

tions when homozygous, and minimum spontaneous rates of mutation) agree with general experience in genetics.

Since the spontaneous mutation rate is not known directly for any human population, both reports adopt the device of estimating the additional dose of radiation that would be required to double the existing rate, whatever it may be. The British and American geneticists estimate that citizens of the two countries now receive during the first 30 years of life, from all sources, a total average dose of 3 to 4 roentgens to the reproductive cells, of which fallout from present weapons-testing programs contributes about 0.26 roentgen. Both groups reach an estimate of 30 to 80 roentgens over the first 30 years per person, as the additional dose required to double the present rate of mutation.

Assumptions

Now, it is these doubling estimates that determine the recommendation for protecting future generations from mutation damage due to increased radiation; and it is thus necessary to examine them more closely. Both reports present them frankly as informed guesses. The British report cites the sources of experimental evidence on which its guess is based. The evidence is derived from three plant and two animal species, of which one is an insect, *Drosophila*; the other is a mammal, the laboratory mouse, on which work for this purpose has only recently been started. There is no evidence from man, and figures for the risk of human mutation thus have to be obtained by extrapolation.

The uncertainty of the method is recognized by both groups of workers. Since *Drosophila* for many years provided most of the evidence, it tended to be used as a standard. But there is a recent report that mouse spermatogonia produce about 15 times as many mutations per unit of radiation as do the comparable *Drosophila* cells (*4*). This is based on tests of only seven genes, which may not be representative. But in general there seems to be little doubt that mammals are more sensitive to radiation than insects. This leads to the tendency, now evident, to lower the doubling dose for mammals and men when extrapolating from existing data on insects. This leaves the problem of extrapolation from mouse to man, and for this there is no real solution at present, except to get more and better data from experimental mammals and, especially, to compare the thresholds of mutation-induction of mammalian genes that have higher spontaneous mutation rates with those that have lower spontaneous rates. Until this is done, averaging the rates for different genes will have very limited usefulness.

But the real difficulty comes in extrapolating from mutation rates to estimates of genetic damage to populations. This is because the eventual effect of increases in the mutation rate depends on two factors that are virtually ignored in the reports. One of these is that the effects of mutations depend not merely on the quantity but on their qualitative character as well. The other is that the effects depend on factors governing the biological structure of the population as determined essentially by its past responses to evolutionary forces acting upon it, the chief of which is natural selection.

The implicit assumption that radiation-induced mutations are like those which occur spontaneously is open to question. It is true that both kinds are usually deleterious when they are homozygous; but there is evidence that a large category of mutations induced by ionizing radiations represents losses of function, less reparable on the whole than those that arise spontaneously. It cannot be assumed without further evidence that both face the same evolutionary fate.

Reaction of Populations to Mutations

Concerning the reaction of the population to new mutations injected into it, there is now a good deal of evidence from experimental populations that many mutations with deleterious effects when homozygous are retained in the population because they *increase* the fitness of those members who carry only one deleterious allele, that is, of heterozygotes. Most wild populations of house mice in the United States contain, in high frequencies, animals that transmit (that is, are heterozygous for) lethal alleles responsible for abnormalities and death before or near birth. Yet such heterozygotes are actually favored both by natural selection and by a peculiar mechanism by which more lethal alleles than normal ones are transmitted to the next generation. The fate of such mutations in the population—retention in this case—obviously depends not on their effects when they are homozygous but on their effects when they are present singly in an individual. Natural selection in such populations is of first importance; mutation frequency is secondary.

This fact was first established by Dobzhansky for many wild populations of *Drosophila*. In these populations, deleterious genes have high frequency but are tolerated and retained because the population adjusts itself to its environment by means of a network of interactions among many genes, coadapted to work well with each other when present singly in the heterozygous state, which is their normal state in the population.

This view of population structure has recently been put to experimental test by Wallace at the Long Island Biological Laboratory at Cold Spring Harbor, New York. Populations of flies subjected to continuous radiation for 120 generations (and thus to increased mutation frequency) still adjust themselves by natural selection, and some have even increased in evolutionary fitness. This is due not to the effects of new mutations when homozygous (such homozygotes are probably nearly always eliminated) but to increased opportunity for new gene combinations to arise, some of which are superior to those displaced.

This is a test, not of the effects of radiation-induced mutations (which are prevailingly deleterious) but of a general theory of population structure which must be taken into account in any extrapolation from radiation effects upon mutation frequency to ultimate effects on the fitness of the population as a whole (*5*).

Next Steps

Although the reports I have cited mark a milestone in bringing to public view the relationship between public policy and research in radiation genetics, it is obvious that they are beginnings only. The next steps should deal with implementation.

Those concerned with research can be most readily conceived and carried out, for methods and guiding principles are available; and, thanks both to what the reports say and what they omit, we know now where some of the chief gaps in our knowledge lie. One of these is how populations, as well as individuals, react to increased mutation frequency; and this is intimately related to the evolutionary forces that form the genetical structures of populations. In filling this gap, experimental populations can serve as models by which principles rather than applications can be tested. In addition to the rapidly breeding forms such as *Drosophila,* we can move closer to human models by dealing with mammalian populations, especially with laboratory rodents. Of these, the house mouse is most suitable because of its short generation time and the advanced state of knowledge of its genetic system.

But it is data concerning the fate of new mutations *in populations* that are needed as well as measures of mutation rate.

Extrapolation from mouse to man is probably less risky than extrapolation from animals or plants lower in the evolutionary scale. The justification for this is found in evolutionary considerations. A species may be conceived as a community of genes derived by common descent, circulating now and in the past by reproductive communication within the species. The genes of related species give many indications of having common properties. The general mechanism of heredity is much the same in all mammalian species. Similar mutations such as those to albinism, white spotting, color patterns, and eyes and those affecting the spinal column and central axial system, including the brain and the tail, have been recorded in man and other mammals as diverse as mouse, rat, guinea pig, rabbit, dog, cat, cattle, and others. These show, in addition, common patterns of embryonic development and growth, of which the similar effects of the genes are a reflection. The mammalian community of genes, or at least a significant part of it, embraces more than a single species. We can therefore, in a properly limited sense, deal with genes like human ones which are accessible in other mammalian species.

Implementation in terms of public policy is more difficult and will depend on how long a view of the human future is to be encompassed. For the long view we must await the outcome of observations on populations in the afore-mentioned sense; for the short view, the safest assumption at present is that the best level of *controllable* radiation is none at all, and the best counsel is to permit additional exposure only when a compensating benefit (as from medical exposures) is to be expected.

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Uses of Atomic Radiation and Energy

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ATOMIC energy, whether we like it or not, is here to stay. I would like to stress this point in both the military and civilian areas. Unless enormous strides are made in international relations, the use of atomic energy in weapons is here to stay. From the civilian viewpoint, the very power of the radiation-detection technique gives assurance that atomic energy is here to stay as a research tool. For example, radioisotopes give us a means for handling dilution factors as high as 10^{15} or perhaps 10^{17}. Thus atomic radiation is an enormously sensitive and delicate detection method.

Even though my own background has been mainly in the field of atomic power, I believe that, in the historical sense, the use of atomic energy or atomic radiation as a research tool will in the future be considered much more important than its use as a source of energy, for atomic radiation is unique as a research tool; as a source of energy, it is merely another form of fuel. Since atomic energy is here to stay, our problem is to learn to live with it.

Need for a New Source of Power

It seems to me that it is only a matter of time until we will not only use atomic energy as a primary source of power, but will be dependent on it for power. The total population of the world was comparatively low from prehistoric times up until about the 16th century. From that time until the present, it has been growing very rapidly. Not only has the population been growing very rapidly, but the use of energy per person has also been growing very rapidly, particularly since the industrial revolution. The total demand for energy is the product of the population curve and the curve for use of energy, both of which are growing at an ever-increasing rate. It is the enormous demand for energy that is obtained by projecting these two curves into the future that makes us interested, first in atomic energy, and finally in solar energy, as ultimate sources of power to maintain and increase our standard of living.

The projected need for energy in the United States alone is shown in Fig. 1. At the bottom are the calendar years from 1900 to 2000. Plotted against years is the consumption of energy from various fuels. Actual values are given for the years up to 1953, and projected demands are given for subsequent years. The important thing is that the use of coal, as coal, has been almost constant, and this trend will no doubt continue. Hydropower is represented by the thin sliver at the top of the curves and does not promise to make much contribution to the enormous demands of the future. The significant increase has been in the use of fluid fuels, oil and gas, for the automotive, airplane, truck, and diesel industries. This is the area in which we are going to run out of energy first. Even with new discoveries, it is predicted that this period will come about 1970 or 1980. At that time, it is very likely that the oil industry will begin to convert slowly to using fluid fuels from coal, for it seems very unlikely that we will give up the use and convenience of fluid fuels as energy sources. Since the available energy from the other sources decreases, it seems likely that uranium will begin to come into the picture. This will not have a revolutionary effect on either the coal or the oil industry. It will be a gradual shift from the scarce sources to the more abundant atomic energy from uranium.

Fuel costs in different sections of the United States are shown in Fig. 2. They run from about 10 cents to about 35 cents per 1 million British thermal units. Even in the United States, where fuel costs are surprisingly low, the lowest costs are in the Texas-Gulf region. Strangely enough, industry tends to concentrate in the areas of least cost, even though it is clear that the cost of fuel is only a small part of cost of running an industry. If we turn to atomic energy, we find that the transportation costs for uranium fuel would be very much lower than those for coal, oil, or gasoline. Since there is as much energy in 1 pound of fissionable uranium as there is in 2.6 million pounds of coal, transportation of uranium fuel, in principle, ought to be cheap, and transportation costs should be reduced. Now, if we turn to the world picture, we could draw a similar map of fuel costs. However, the variations would be even greater because there are many regions of the world where fuel costs are really very, very high compared with the costs in the United States.

However, there is no use in turning to uranium and developing it as a source of energy unless there are sufficient supplies to have real industrial potential. Figure 3 shows the estimated fuel reserves in the United States. The unit used, Q, is equal to

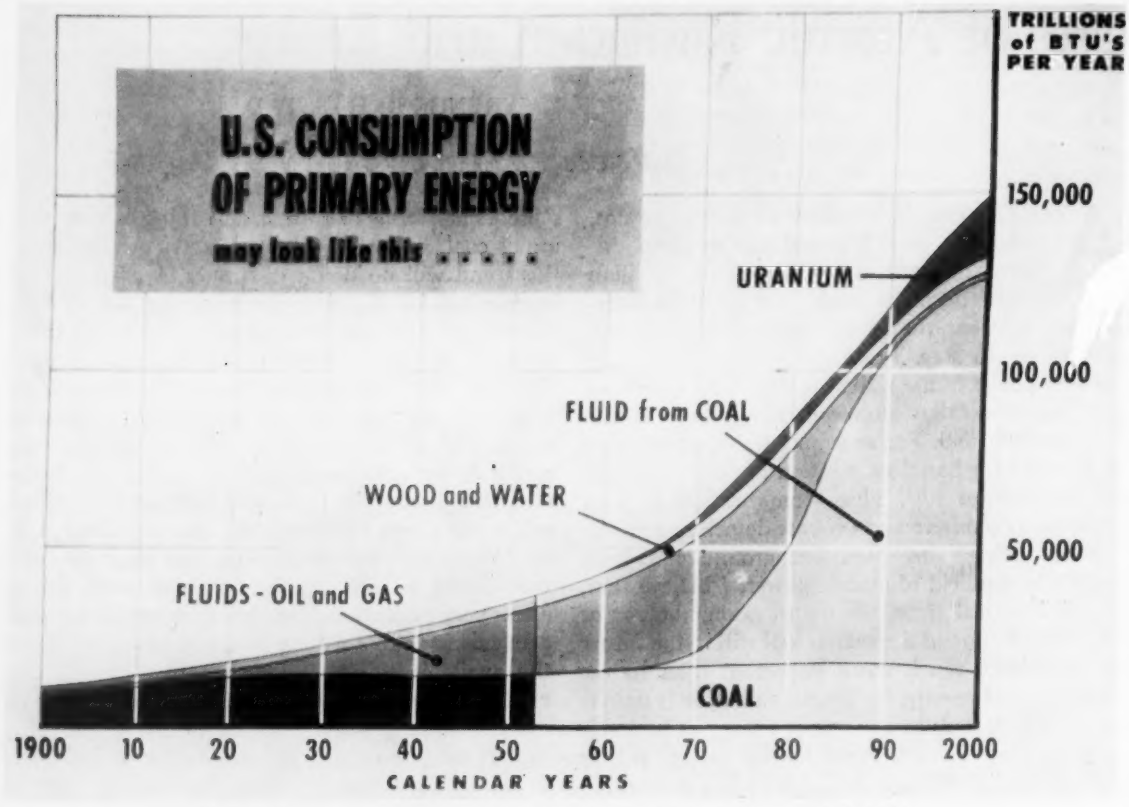

Fig. 1. Consumption of primary energy in the United States, 1900–2000.

10^{18} British thermal units. The important values are the relative amounts of energy available from the different fuel sources. At the top is coal, with an assured supply of 7.6 Q units. Petroleum, you will note, is low with 0.2 Q unit. Even oil from shale, which is considered to be available in rather large amounts, is only 1.2 Q units. Natural gas is about the same as petroleum, with 0.2 unit. Now, if we turn to uranium and thorium, we jump to about 1500 Q units, a figure much greater than the total energy potentially available from coal and oil combined.

To make use of the energy in uranium, we must build nuclear reactors. Reactors produce many different things. All reactors produce neutrons, which can be used for making explosives, for making more reactor fuel, for making isotopes, and for direct use in research. All reactors produce heat that can be used to generate electricity, to operate propulsion devices such as those used in ships, and to heat buildings; ultimately, the heat can and probably will be used for process heating where large quantities of heat are needed and where the cost of the energy is important. Finally, the fission products, which are themselves radioactive, can be used to give us gamma rays for food sterilization and for use in the chemical industry for controlling chemical reactions. The latter use is extremely important because it gives to chemists for the first time essentially two degrees of freedom in chemical reactions. Ordinarily, chemists work with temperatures that can be varied at will. Now they can vary both temperature and ionization—that is, the radiation effects.

Atomic Products

Let us consider the uses of atomic energy, starting with radiation. Many radioactive isotopes emit gamma rays, which penetrate many solid substances, just as x-rays do. Radioactive cobalt, which emits gamma rays, is used for detecting flaws in castings or forgings. This is not a particularly new use, but compared with radium, cobalt-60 and other radioactive isotopes are extremely cheap, and this method can now be used on a large scale. Before reactors were available, this technique could be used economically only in very special cases.

Radioisotopes are used in studying the mechanisms involved in the uptake of fertilizer. Radioactive phosphorus may be put in the soil. It is taken

up by the plant, and the amount of radioactivity that appears in the tissues can be measured. The rate of uptake, the form in which the fertilizer is utilized, and other factors can be studied accurately. This was one of the first uses of isotopes, and it is the type of use that is increasing very rapidly in industry at the present time. It is one of the most important uses of radiation.

The use of radioisotopes in medicine is striking. There is a "blood-brain barrier" in the brain that prevents material from coming from the blood into normal brain tissues, or delays its coming. But if a person has malignant tissues, this barrier changes, and there is a chance of bringing radioactive chemicals into the malignant tissues. Once this has been done, it is possible to use radiation-detecting instruments to locate the tumor in the brain before surgery.

In the automotive industry, the measurement of friction and the testing of lubricating oil are very important. In order to test a piston or piston ring for wear, it is normally necessary to run an engine for a very long period of time and to use a micrometer to measure the amount of wear on the piston rings or cylinder walls. By the new method, the piston ring is exposed to neutrons to make it radioactive. After a very few minutes of operation, enough material has rubbed off the piston ring and the cylinder wall to appear in the oil, where it can be detected by the sensitive detection devices that are available. With this technique, it is possible to increase the number of measurements that can be made and thus to determine rates of wear and to test materials and oil very very rapidly. Here is where dollars are being saved at the present time. This method is no longer in the research stage. It is a practical use for radioactive isotopes.

It has been discovered that vegetables—onions or potatoes—can be preserved—that is, kept from sprouting—by exposing them to large doses of gamma rays. Onions that have been exposed to gamma rays last more or less indefinitely. This method of food preservation would have particular importance to the Army, and the Army Quartermaster Corps is exploring this field very actively at the present time. You can see the implications of this as the world moves into the era of large population mentioned in a previous paragraph. If we are going to have to feed a large population, anything connected with food storage and preservation is of great importance.

Atomic Power

Nuclear reactors are used both for research and for producing power. Figure 4 is a diagram of a

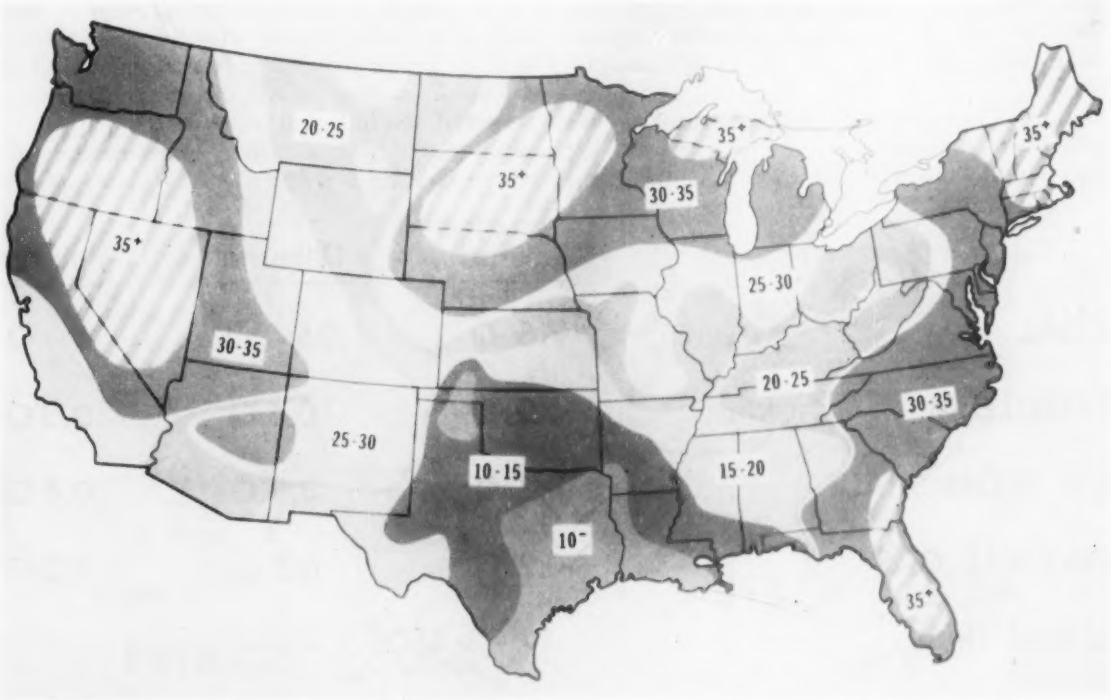

Fig. 2. Costs of fuel in cents per million British thermal units in various regions of the United States.

nuclear power plant. The reactor is in the center of a large pressure vessel. When the reactor is operating, it gets hot and heats the water that surrounds it. The water runs through pipes to the boiler, where steam is generated. The steam is used to drive a turbine, which in turn runs a generator to produce ordinary electric power. Figure 4 is a schematic diagram of the way in which one obtains ordinary electric power from a reactor. Most of the process is strictly conventional engineering practice.

In adopting atomic power for civilian use, the first requirement is that it must cost no more than the electric power obtained from other sources. Figure 5 shows the conventional, coal-burning, central power station at the top, with its coal-handling equipment, boilers, turbines, and distribution system. If you turn to atomic energy, you will note that you replace the coal-handling equipment and the boiler with a reactor and boiler. All the rest of the equipment remains the same, and all the costs that go with the distribution of the electric power will remain the same. The only thing we have in atomic energy is a potentially cheaper fuel. The current cost of generating power might be taken at—let us say—8 mills per kilowatt hour. Of this, about 2 or 2.5 mills represents the cost of fuel itself. With atomic energy, we hope we can reduce the 2 mills to 1 mill, or cut fuel costs in half. If we do this, we will make a significant reduction, but not a revolutionary reduction, in total power costs. Atomic energy, then, is more a guaranty against rising energy costs than a promise of significant reduction.

One has to consider the entire system when one talks about the economics of atomic power. We start with raw materials, which must be fabricated into fuel elements. The fuel then goes into the reactor, where it is used to produce heat and plutonium, among other things. After a time, the fuel becomes contaminated with fission products, and it must be removed for chemical processing, after which one gets back plutonium, if you like, for military or civilian development purposes. Attached to the reactor is a cooling system, or a steam generator if we want to extract electric power. If electric power is wanted, we must install an electric generator that will give us a certain number of kilowatt-hours. For each gram of plutonium that is produced, about 4000 kilowatt hours are obtained from the electric generator if things are working normally, and this power has a certain cash value. It has one cash value for the United States, where power costs are low because of our relatively abundant resources of coal and oil; it has a higher cash value in the United Kingdom and elsewhere abroad.

We have two uses for the plutonium—a military use, for which the value is high (I cannot give any particular figure on that), and a civilian use for which the value is somewhere in the neighborhood of $15 per gram. Let us compare the results of operation in the United States and in the United Kingdom. The sale of 6000 kilowatt hours of electric power would bring in about $25 in the United States and about $50 in the United Kingdom, where power is more costly. Now add up the economic balance as the business man sees it. Note that in the United States, when you take the $25 that you get for the electric power, and add about $15 for the civilian value of the fissionable material produced, you have $40 with which to pay for the entire plant. Now, if you make estimates of the cost of building and operating this plant, you will

	Assured	Ultimate	Putnam
COAL	7.6 Q	25.4 Q	6 Q
PETROLEUM . . .	0.2 Q	0.4 Q	0.3 Q
OIL FROM SHALE . .	1.2 Q	3.0 Q	0.4 Q
NATURAL GAS . . .	0.2 Q	0.3 Q	0.2 Q
U and Th	- - - - 1500 - - - -		

Fig. 3. Fuel reserves of the United States (1 Q is equal to 10^{18} British thermal units; it is equivalent to 40×10^9 tons of coal, 170×10^9 barrels of oil, 10^{15} cubic feet of natural gas, or 17,000 tons of uranium or thorium).

find that it just cannot be done with this income of $40. This is what we mean when we say that atomic power is not economic at the present time in this country. Foreign countries are clearly much more interested in atomic energy than we are.

Countries such as the United Kingdom and the Soviet Union, with their military programs, can credit themselves, at least mentally, with possibly $100 per gram for the military value of plutonium. They can credit themselves with about $50 for the electric power generated, and this gives a total of approximately $150 for the value of the output of the plant. Thus the higher figure may be used to estimate the permissible cost of the plant and operation.

This different aspect is significant, for we read in the newspapers these days that the United States is behind the United Kingdom in atomic power plants. It should be realized that the economic situations in which the programs are proceeding in the United States and in the United Kingdom are entirely different. The responsible people in each country have designed a program for the development of atomic energy that is suitable for the situation in which that particular nation finds itself. The fact that our program is different from that in the United Kingdom should not cause concern. I have tried to stress the fact that atomic power is not economic in this country. If, for reasons of international politics, it is desirable that this program be moved along faster than straight economics justify, then in my opinion there is no way out but to have the government in some manner or another expedite the program. The important thing is that a clear policy decision should be made concerning the objectives of our atomic-energy program. Once the objective is clear, our technical personnel and scientists can and will plan an effective program to attain that objective.

A large number of reactors are being talked about and studied in this country. Here is why this situation exists. One type of reactor, call it type A, seems to be the closest to being economic and might be the type that one would choose for development. But this could be dangerous from the long-range point of view because another type—B, which has a higher cost at the present time and is farther from being economical—may have a development potential that will bring it well into the competitive range in the future. As an example, I would say that type B might very well be the so-called "breeder" reactor, which is extremely difficult to build and develop, but which does have the promise of very low—almost vanishingly small—fuel cost in the distant future once it has been fully developed.

Actually, there are more than just two types of

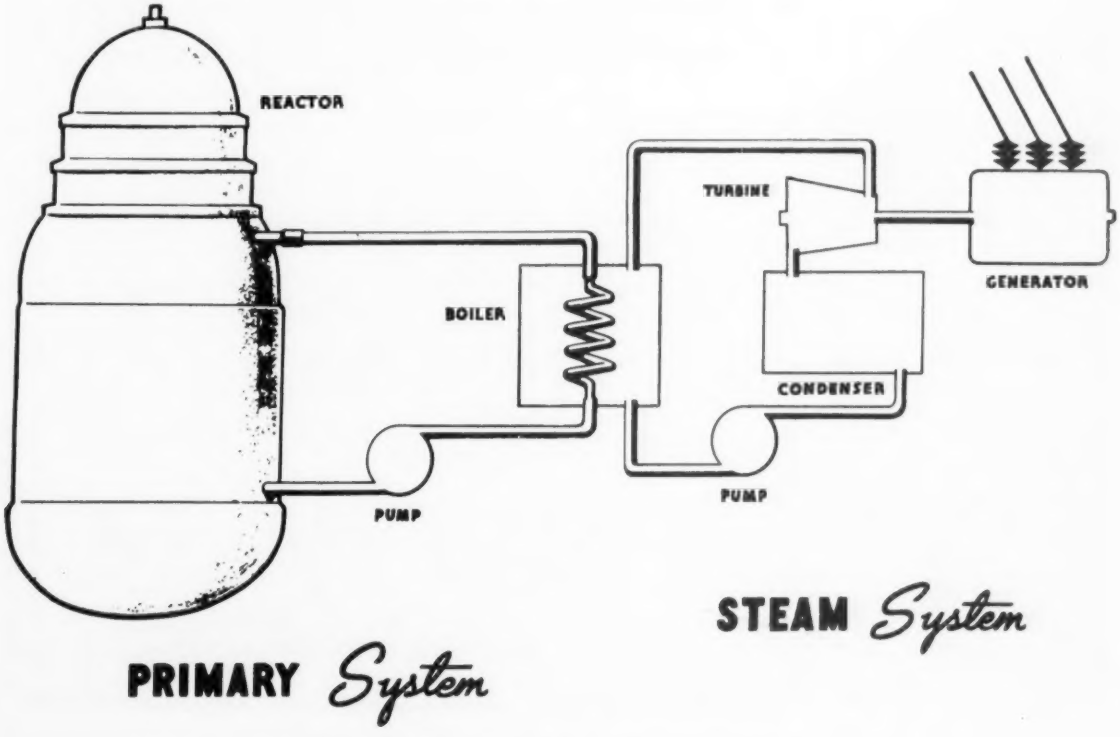

Fig. 4. Nuclear power plant with water cooling.

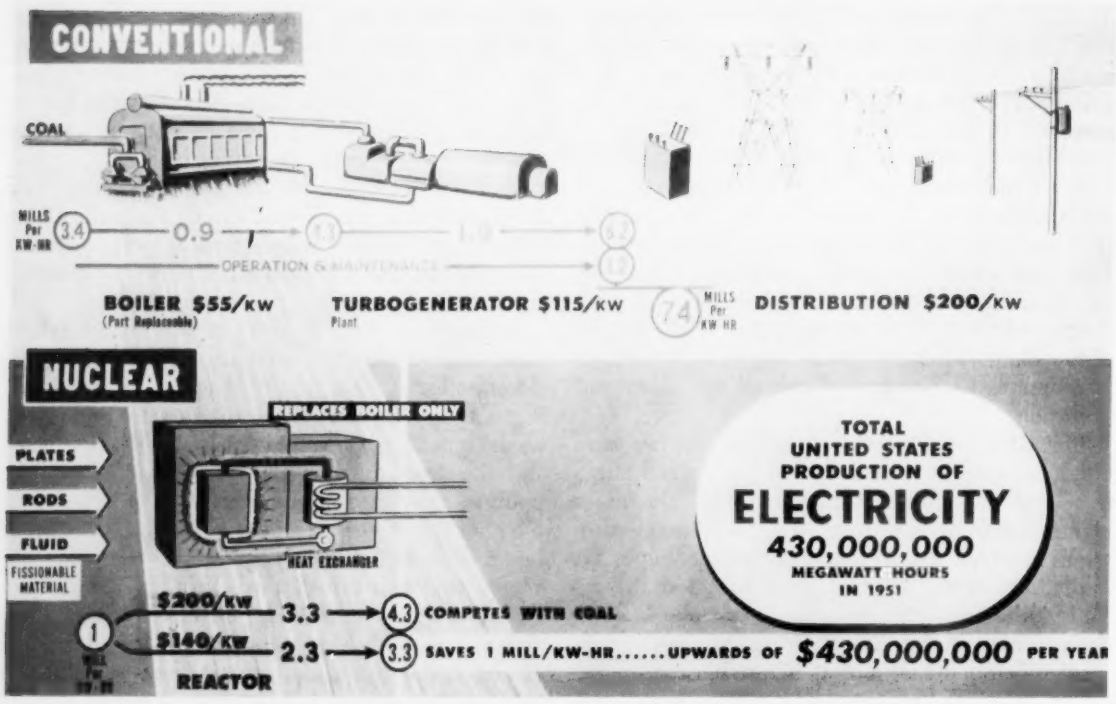

Fig. 5. Allowable reactor costs. Amortization over a 20-year period is assumed. The fuel cost for the reactor is based on the following assumptions: (i) cost of uranium at least as great as it is at present to the AEC; (ii) reasonable burn-up of natural uranium; (iii) 25 percent thermal efficiency; (iv) no credit for plutonium produced; (v) no value for irradiated fuel.

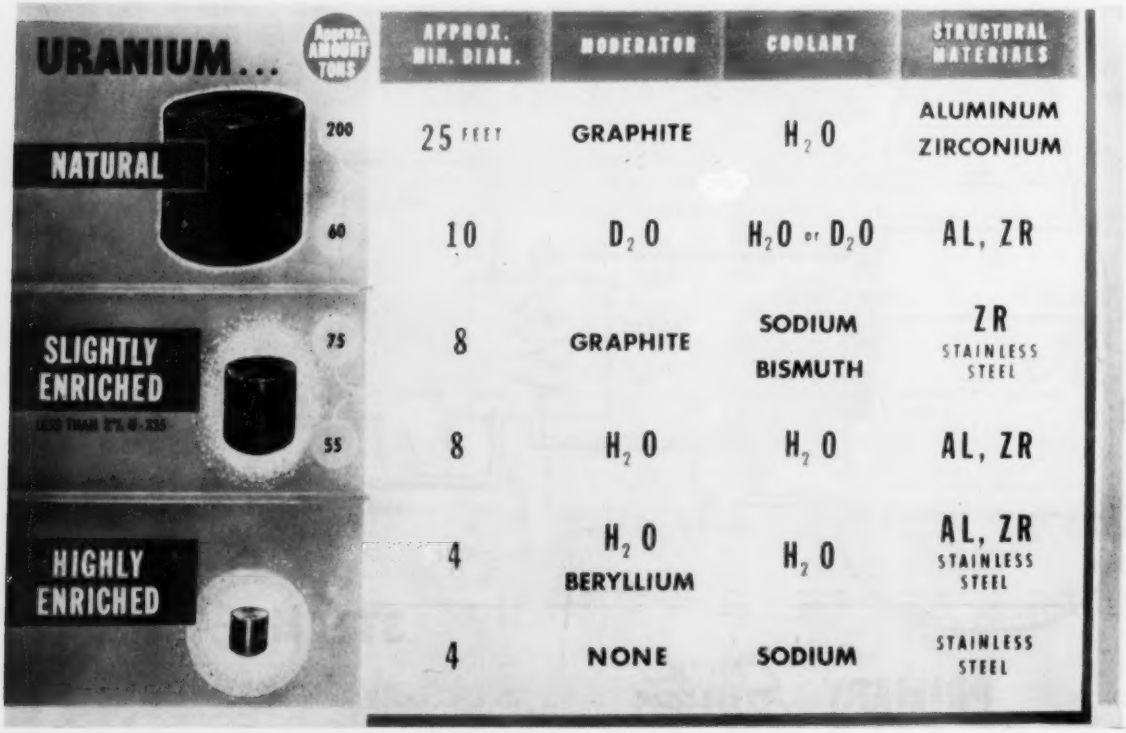

Fig. 6. Reactor core sizes and materials for hypothetical designs of power reactors to produce about 100,000 kilowatts of electricity.

reactors (Fig. 6). One type uses natural uranium, which gives a cheap material to work with but which requires a large reactor. A second type uses slightly enriched uranium. A third type uses highly enriched uranium, where one has a very small and compact reactor, such as is used in the military applications, but where the material itself is very expensive. In addition, there are variants that use different moderators, coolants, and structural materials—and all combinations of these different factors. These various combinations result in different kinds of reactors. There are some dozens of different reactors being considered. You might think that this shows lack of planning and decision, but it is interesting to note, for example, that in the automobile business there have been some 2900 different plans for automobiles devised and tried out on the market. Thus we have a long way to go in the reactor business before we are even in a position to pick the best one—and we have not yet picked the best one in the automobile business either.

The different types of reactors we are working with fall into five general families. One is a family we call "homogeneous reactors," which have fuel elements whose fuel is suspended in a liquid of some kind. Second, we have the liquid-metal family, where we use liquid metal, such as sodium, for cooling because the heat-transfer problem is so great that water is inadequate. Third, we have graphite-moderated reactors, usually water-cooled. We also have a marriage of the second and third families in the sodium-graphite reactor. The fourth and fifth families are represented by the light water reactors and the heavy water reactors. All these different types of reactors are being studied in the United States, and our program covers all the types. Most foreign countries have chosen to specialize in one or two of these five different families. I think I can say that, as far as technology is concerned, and knowledge of reactors in general, the United States is well ahead of any competition of which we know. In the commercial use of atomic energy, there is again no doubt, and there should be no embarrassment in the fact, that the United Kingdom is ahead of us.

Conclusion

In conclusion, consider the question, How close are we to the use of atomic energy for generating electric power? The capital costs of some of the reactors that we have now built or are building are very much higher than the costs of diesel or coal-fired plants of the same electric capacity, but the costs of a few of the reactor plants are close enough to the costs of conventional plants so that it begins to look as if the reactor plants might be economically successful. However, we have a somewhat different picture if we consider over-all power costs rather than capital costs, for the fuel costs of an operating reactor give it an advantage over conventional plants.

If we consider the cost of producing power in mills per kilowatt hour, we find that some of the atomic power plants now planned or under construction promise to become very competitive. The somewhat newer plants now planned and which will be constructed may begin to be competitive with the conventional coal plants. Having been through this several times, I would add that, practically, the cost of the plants planned will no doubt be higher than the sponsors now think. As those associated with these projects know, they always cost more and take longer than is expected in the beginning. Perhaps this generation of atomic energy plants will not be competitive. This merely means that the lessons learned will enable us to make such improvements that perhaps the next generation of plants, 5 to 10 years from now, will be economical.

What We Most Need To Know

LAURENCE H. SNYDER

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I AM sure that if 20 people were asked to write on "What we most need to know," they would produce 20 quite different papers. The suggestions made would depend largely on the point of view and the training of the individual. I will naturally be more familiar with some of the topics I discuss than with others, and I will have to draw considerably on the thinking and writing of my colleagues in various fields. Moreover, it will be realized (and by no one more clearly than by myself)

that the answers to some of the problems I suggest may already be known but are sealed in classified secrecy.

The unsolved problems related to the effects and uses of high-energy radiation lie in the fields of the life sciences, the physical sciences, and the social sciences. I shall consider briefly each of these areas.

Life Sciences

The able and authoritative summary reports on the biological effects of atomic radiation by the committees of the National Academy of Sciences which studied this question expose our serious deficiency of many fundamental data. This deficiency is perhaps nowhere so conspicuous as in the area of genetics, and particularly the genetics of the human species.

In genetics we need knowledge, both more extensive and more precise than is currently available, regarding the relationship between radiation dosage and induced mutation rate. In particular, we must ascertain the relative frequencies with which mutations resulting in different degrees of impairment (or improvement) of viability occur in consequence of radiation. Information of this kind is essential for assessing the potential genetic effects of artificial radiation.

It is difficult to envisage how this information could be obtained in any adequate degree by studies on man alone. We must therefore investigate the problem exhaustively in a sufficient variety of experimental animals, including especially mammals, at the same time that we vigorously pursue related investigations of the human species. It should then be possible to evaluate data from the human material in the light of the range of variability found in the animal studies.

Among the problems urgently requiring investigation is the question of whether the frequency of point mutations is still linearly proportional to radiation dose at extremely low doses. It probably is, since the doses already studied are low enough that a given germ cell exposed to these doses is traversed by only one ionizing track over a long period; and from these doses on down to zero it would seem that the relationship must surely remain linear. But this should be tested experimentally. Although there is apparently no threshold effect in the radiant environment, there may well be an upper limit to mutability in the gene itself, and the limit may differ from gene to gene. The implications in this concept are so important in estimating genetic damage that the possibility must be investigated, and any limits for a representative sample of genes identified.

Most experimental work on radiation-induced mutation has been done with genes that either in heterozygous or homozygous condition have clear-cut individual effects. It may well be that the induction of mutations which, when heterozygous, individually produce effects that are not readily discernible, is of equal or greater significance. Most quantitative variation depends at least in part on the cumulative effects of genes of this kind. The detection of radiation-induced mutations in this class of genes would require careful measurement of such quantitative traits as growth rate, disease resistance, physiologic and reproductive fitness, and, of course, life-span.

Work of this kind would have to be done chiefly on laboratory animals, but it may be feasible under certain circumstances to make comparable investigations on human material. It is conceivable that a further study of the Hiroshima-Nagasaki material from this viewpoint might yield valuable information, since the original conclusions were based on the change in sex ratio due to sex-linked recessive mutants and on visible congenital anomalies resulting from dominant mutants. Observations directed to the life-spans of the children of exposed parents, their age mortality from all causes, and the number of their living progeny might be of value. There is, however, a grave question whether adequate controls could be obtained for such a study.

Serious consideration should also be given to the undertaking of new and intensive studies from this viewpoint on the children of radiologists and of other persons who are exposed by occupation to ionizing radiations. Direct survey, rather than questionnaire, methods would almost certainly be required, and again the question of competent controls is of primary importance. Perhaps the progeny of the like-sexed sibs of the parents would best serve this purpose.

We know very little about the spontaneous mutation rates of human genes. From direct observations of the occurence of new dominant abnormalities, and from equilibrium estimates for sex-linked recessive anomalies, mutation rates have been inferred for a few loci in man. The reliability of the rate estimates obtained in these ways is somewhat in doubt, because they have concerned genes, the nonmutated alleles of which cannot be identified. Only when we can identify the normal allele itself are we sure that the mutations we observe are all at the same locus. This identification can be made, of course, for such genes as those that are responsible for a number of human blood-group differences.

It has been suggested that if an extensive cooperative study were set up in a number of blood-

grouping laboratories, it should be possible, by blood-typing a sufficiently large number of children and their mothers, to obtain relatively secure information on mutation rates at a number of specific loci in man. This would be a very large-scale operation, of course, and it would be expensive; but I would hope that funds could be made available for carrying out pioneering studies of this nature. It is essential that we determine the spontaneous mutation rates of as representative a sample of human genes as possible.

The recently developed methods for separating individual human cells and growing them as pure clones in tissue cultures open up a remarkable new approach to the problems of mutation. The possibility emerges of studying by these methods both the spontaneous mutation rates of human genes and the induced rates under varying doses of ionizing radiations. These rates, once estimated, could be directly compared with mutation rates established by the same techniques for experimental organisms, thus testing the validity of extrapolating to human terms the mutation data that are derived from breeding experiments with laboratory species.

It should be possible to adapt to human tissue culture the screening methods so successfully used with microorganisms—namely, the discovery of mutant cells through the use of minimal media, antibiotics, and antibodies.

If we are to assess the potential genetic effects of increased human exposure to radiation, it is, of course, not enough to know what the effects of a given increase in mutation rate would be and how much artificial radiation would be required to produce this increase. We should have at least a fair idea of how much radiation the species is likely to be subjected to during the foreseeable future.

The National Academy of Sciences' committee on genetic effects undertook meticulous computation of the probable average per capita dosage received annually from natural radiation, medical x-rays, and weapons tests, respectively. For the future, we may assume that natural radiation will remain constant and that the vigilance of physicians will prevent an appreciable increase—and may even result in a decrease—of radiation from medical x-rays. What we may expect from weapons testing, however, would appear to be an unknown quantity.

In the National Academy of Sciences' report, estimates of the probable genetic effects of radioactive fallout from weapons tests are carefully and consistently qualified with such phrases as "if the testing of weapons continues at substantially the same rate as in the past . . ." and, at one point, ". . . if weapons testing were continued at the largest rate which has so far occurred" (that is, in 1953 and 1955). I must note here that the average testing rate for 1953 and 1955 appears to have been 3 times the average rate for the other 3 years of the quinquennium, and it is almost certain that the "largest rate that had so far occurred" when the report was prepared has itself been more than doubled during 1956. What may we expect in 1960? in 1980?

The National Academy of Sciences' committee on genetic effects of atomic radiation has suggested the institution of a national system for keeping records of radiation exposure, under which there would be maintained for every individual a complete history of his exposure to x-ray and gamma radiation. The suggestion has been criticized as being idealistic and not attainable. It should certainly be possible, however, to set up a pilot sample of 10,000 or 100,000 individuals who would cooperate in such a study. Even if the people cooperated only on a 6-month or a 1-year basis, the sample could be progressively changed, and valuable results could be obtained. It is to be noted that similar cooperation was recently achieved on a large scale in the study of smoking and cancer.

There are strong recent indications that in addition to long-term genetic effects, the life of the individual himself may be shortened by radiations, perhaps by as much as 10 or 15 days for every roentgen unit received. This relationship seems also to be linear, so that the accumulation of very small doses can produce the effect. Obviously we need a thoroughgoing study and reanalysis of the use of radiations for diagnostic purposes. This is a problem that should be tackled immediately and vigorously by the medical profession.

Prominent among our needs for the future is a better understanding of the mechanism of radiation damage, both pathologic and genetic. Coupled closely with such need is the importance of possible protection of cells and tissues against damage. We know as yet but little about the specific effects of radiations on large molecules and complex compounds. Nevertheless, studies already under way are yielding surprisingly good results on the feasibility of protection from injury and even of recovery as a result of postexposure treatments. Reducing compounds, tissue transfer, shielding of specific parts of the body such as the spleen, and antibiotics have all given promising preliminary results. Further studies along these lines are urgently needed. It is not too much to hope that protection and treatment may become feasible even for mutagenic damage: an outcome devoutly to be strived for.

Researches along the lines of protection from

mutagenic radiation damage must not be confined to studies of this type of mutagen alone. Chemical mutagens, as well as spontaneous mutations, must be carefully studied in order to put the problem in its proper perspective. In fact, any basic study of any kind in genetics, physiology, biochemistry, and similar fields may suddenly become of unexpected value in elucidating present and future problems. One cannot always specify the particular line of research that will lead to the fulfillment of the apparent needs of the moment, nor can one always foretell the needs of the future. In the hands of a capable investigator, basic research is always of value.

On the border line between the life sciences and the physical sciences lies the rapidly developing problem of the safe disposal of radioactive wastes. For 10 years now radioactive isotopes have been produced and widely distributed. Larger and more intense sources of radiation are increasingly demanded. The accumulation of fission products is increasing all the time, and, although the initial decay is rapid, the long-term remaining radioactivity poses a real problem of disposal. It is essential that isolation or dispersal be accomplished without hazard to living things. Two general methods have been suggested: disposal by dilution, as in the ocean, and disposal by concentration and storage. Both methods have serious drawbacks, and much research is needed to reach an equitable conclusion. It is estimated that within 25 years there will be an accumulation of some 200 million gallons of waste, and within 50 years more than 2000 million gallons. By that time we shall be faced with the problem of safe disposal of more than 1 million gallons a day.

We are as yet unable to predict the effects which ocean dispersal would have on the sea and its resources. Dispersal in the sea is slow at best, but there is no doubt that the radioactive content of the oceans would be raised to a considerable extent in a somewhat uneven manner. We obviously need a lot of basic research, long-range rather than temporal, on the physics, chemistry, and geology of the sea. The fine studies being carried out as part of the International Geophysical Year are tentative steps in the right direction. We need more intensive research on the biology of various marine organisms. Such studies must be world-wide. They must be started at once, before the ever-increasing radioactive contamination of atmosphere and sea makes some of the most basic experiments impossible.

We need also, of course, geophysical and geochemical investigations of the land, looking toward adequate selections of sites, both for nuclear reactors and similar facilities and also for possible waste disposal. The safe transportation of intensely radioactive substances requires continuing research. And, naturally there must be never-ending studies of the socioeconomic and the public-health aspects of the production of radioactive materials. Finally, there is reason to hope that, rather than their being only a nuisance, radioactive wastes may eventually be turned to practical industrial uses. Chemical synthesis, involving polymerization, halogenation, oxidation, and other molecular rearrangements appears to offer possibilities. Researches along these lines are among our great needs of the future.

Physical Sciences

An important objective of the future in the physical sciences appears to be the taming of the thermonuclear reaction: the production of industrial power from fusion reactions involving the light elements such as hydrogen, deuterium, lithium, and beryllium. There is no obvious fundamental principle of physics that stands in the way of this achievement, yet the important breakthrough that will make possible the construction of thermonuclear plants for the controlled production of power has yet to occur. One of the difficulties is, of course, that exceedingly high temperatures are involved in initiating the reactions, and no container is known that would not melt at the temperatures of millions of degrees centigrade which bring about these fusions in the stars.

By enormously increasing the speed of atoms of gas, however, high kinetic temperatures without heat as we ordinarily understand it may be achieved, and this fact offers research possibilities. Thermonuclear research at present is fairly well cloaked in secrecy, but certainly many physical scientists are thinking about the problems. Perhaps more progress would be made if the secrecy could be lifted.

There would be no fixed limit to the amount of power obtainable from thermonuclear reactions. There is an inexhaustible supply of deuterium, which occurs in all the waters of the world. Moreover, power from fusion reactions would largely solve the problems of waste disposal, since fusion reactions do not produce radioactive wastes in anything like the amounts that fission reactions do.

Meanwhile, it is necessary to proceed with research on the development of better reactors using uranium and thorium. It is clear that the designs of the present reactors will not be the designs of the reactors that are built a decade hence. Much engineering research must go into improved designs, with emphasis on safety, reliability, and cost re-

duction. Less expensive fuel elements must be found. The chemistry of reprocessing must be carefully studied. The feasibility of recycling the fuel in thermal reactors to increase the extraction of energy must be investigated.

An important need is the technologic development of small "packaged reactors" arranged to drive gas turbines, which do not require large amounts of cooling water. The loss of heat energy may thus be reduced. For arid areas with no readily available sources of coal and oil, such developments could be veritable bonanzas.

Because the ordinary slow-fission reactor can utilize only the small amount (less than 1 percent) of uranium-235 found in natural uranium, a type of reactor is needed that can in some way extract a far greater part of the energy of the total uranium. The answer of the theoretical physicist is the so-called "breeding principle." If high-speed neutrons emitted from the fission of uranium-235 are permitted to be absorbed by a layer of otherwise useless uranium-238 or thorium, these substances should be converted into further fissionable material. Thus a reactor might, in the course of producing industrial energy by burning a nuclear fuel, produce at the same time more fuel than it consumes.

Breeding of this type has been successfully accomplished on a laboratory scale. It is now essential to carry out the research and technology needed to put the principle to practical use on a large scale. Formidable problems involving high rates of heat transfer and development of more stable reactor materials must be solved, but the prize is high, since 1 ton of uranium could provide the energy of 1 million tons of coal, and the needed knowledge will surely be forthcoming.

With large amounts of nuclear energy soon to be available, researches should be undertaken on methods of utilizing this energy in the conversion of ocean water to fresh water for purposes of large-scale irrigation.

One of the most exciting problems of the next few years to the theoretical physicist will probably seem very unexciting to the layman, yet its solution may produce as yet undreamed-of results. The problem consists of working out a theory of elementary particles leading to a real understanding of nuclear forces. The recent discovery of many new particles such as antiprotons, antineutrons, and hyperons has created the need for much high-level research on these particles, but it has not yet provided a real theory of nuclear forces. While the engineers are busily developing new and exciting technologic advances, the theoretical physicists will be equally busy developing equally new and exciting theories. Without such continuing advances in theoretical understanding, technologic advances soon come to a complete standstill.

Social Sciences

Thus far I have spoken in terms of needed knowledge in the natural sciences. Let me conclude with some remarks about the equally important but often neglected obverse of the coin: the necessary advances in the social sciences.

It is difficult today to escape the conviction that our technical skills and scientific knowledge run far ahead of our social understanding. Many thoughtful persons have realized and have expressed in one form or another the dilemma created on the one hand by man's increasing faith in his capacity to expand his technologic and scientific horizons, and on the other hand by his mounting skepticism with regard to his ability to solve his concomitant interpersonal and intergroup problems. Man's struggle with nature is, in general, making satisfactory progress: man's struggle with man is far from being resolved.

It is encouraging to observe that, as the methods and results of science are increasingly appreciated by the public, there is a noticeable lessening of popular faith in magic and superstition. But it is discouraging to note at the same time the small influence that scientific methods and results appear to have achieved on man's basic behavior patterns. Political, racial, and personal intolerances do not seem to have diminished at a comparable rate, and herein lies one of mankind's greatest future needs.

It will take more than new scientific developments, more than increasing technologic advances, to lessen effectively the misery in many parts of the world. There must be moral, social, and economic improvements of comparable stature to parallel the advances in science and technology.

Nuclear energy presents only one of the scientific aspects of the developing forces that are making mandatory the solution of moral and social problems. The rapid increase in the world's population, with its accelerating consumption of energy; the conquest of disease and of other hazards, with the consequent extension of the life-span; the increasing need for greater food production and a more equitable distribution of the food produced, with manifold related problems, all bear witness to the necessity of research in the social sciences.

Yet the loosing of nuclear energy has been the prime factor in the tentative awakening of the public conscience to the urgency of the social problems. Even though fear is at the root of many of the stirrings, we may take hope in the fact that

public apathy has stirred at all. It would be indeed tragically ironic if the same thermonuclear reactions that, by taking place in the sun, make possible our very existence on this earth, should, through our own social bungling, lead to our destruction.

The natural sciences have, within fairly recent times, provided us with basic principles, capable of verification, that are of use in extending man's control over the natural forces and in formulating an adequate understanding of such forces. But utilization of similar scientific principles is unfortunately the rare exception in the realm of man's relationships with other individuals and groups.

The social sciences are as yet in the formative stage, and as a result many activities in human arrangements are still carried out with accustomed conformity to prescientific notions and beliefs carried over from past ages. Extensive research is needed in the social sciences before they have a substantial body of firmly grounded and universally established principles from which wide applications of generalizations may be made.

It is not easy for us to achieve objectivity in problems dealing with ourselves and our fellowmen. We are too often committed to certain notions and beliefs through tradition and through our identification with particular groups. We are often ethnocentric in matters related to ourselves and our identifications. Statements deviating from cherished social beliefs tend to violate our sense of self-identity, our feeling of stability, and even our status in many instances. Objectivity and open-mindedness are difficult to establish and maintain when new discoveries imply changes in time-honored beliefs and practices pertaining to established human arrangements.

Significant problems in social science require plural approaches, often necessitating the aid of the biological sciences. Interdisciplinary efforts are essential and, fortunately, are gradually coming about, in the attack on social problems.

Let me attempt to identify some of the pressing needs in social-science research. We should know precisely how a positive sense of identity that does not need the support of negative stereotypes toward other groups, even other nations, can be developed among children. In a changing world where human societies are less and less frequently closed systems, we should know the principles of acculturation, of the impact of one culture on another, and of the formation of ever wider and more embracing cultural units.

We should know the principles of status and role relations within groups. We need urgently to understand the principles of intergroup relations. Fortunately, recent objective experiments along these lines are beginning to open up fertile new avenues of approach. We should know the principles of developing the capacity to exercise initiating and cooperating functions. We do not yet adequately know the principles underlying adjustment to abrupt changes, sudden dangers, and new fears. We need to understand more objectively the cultural values that have an essential bearing on the manner in which new scientific discoveries are utilized.

The social sciences today are faced with a challenge of the first magnitude. Already they have pointed to the essential oneness of mankind and to the interdependence of diverse groups of humanity in a shrinking world where one nation can no longer remain unaffected by the actions of another. Now it is incumbent upon them to discover the basic principles by which some adequate measure of stability and harmony may be established among human beings. If we do not attain and share in common a core of fundamental premises about the kinds and patterns of social arrangements in political, economic, and cultural spheres which are conducive of a more stable world community, we are doomed to a precarious and shaky existence that is maintained only through fear of total destruction.

I believe that the social scientists are equal to the challenge. I believe that fewer and fewer of them will be content to work on temporal, ephemeral problems. I believe that the increasingly coordinated, interdisciplinary studies that are already beginning will lead to basic, universally applicable principles of stable and harmonious human relations—relations that will not breed friction and conflict at every turn of the road, and relations that can counterbalance the grim consequences that would ensue from indiscriminate and reckless application of the remarkable sources of power now suddenly at our command.

The sociopolitical problems I have just touched on are clearly very immediate ones; they are with us now, and we must face them at once. Hardly less urgent, however, are problems that will confront us in the forthcoming decades as the enormous potentialities of peacetime uses of nuclear energy begin to be realized. We must prepare for those problems now and acquire the knowledge and try to develop the wisdom that will be needed if an age of atomic power is to be a blessing and not a curse.

I think it is safe to predict that within the next two or three generations the provision of an essentially limitless supply of productive power throughout the world will be possible; we must discover ways of assuring that its availability will not be curbed. Entirely new approaches to problems of

international trade as well as of internal economy may have to be discovered for an age when—in present-day terms—the market for nearly every commodity will be a buyer's market. And we may expect this to be true not only of manufactured goods but of agricultural products as well.

Experimental attempts to produce improved crop plants through the acceleration of mutational changes by radiation already show great promise. The application of high-energy radiation to the preservation of food should virtually eliminate loss of food products from spoilage as well as dangers from helminthic parasites in certain meats. The use of radioactive tracer techniques for studying mechanisms of nutrient uptake in plants should lead to greater efficiency of fertilizers. Much research is in progress along these and other lines, and much more, of course, should be done. The consequences, if I am not overoptimistic, should provide the possibility of an abundant food supply for the entire population of the earth.

But there will still be physiographic factors which will very likely make accelerated food production highly feasible in some sectors of the globe and far less feasible in others. That is, in some quarters large surpluses will be readily producible, while in others it may remain difficult even to meet the inhabitants' minimal requirements. Once again, we shall need a fresh and imaginative approach to problems of international trade and of the internal economy of nations if we are to assure that mankind will receive full benefit from the exploitation of nuclear energy.

With perhaps even more confidence, we can predict a considerable and very possibly rapid increase in the availability of leisure for much, if not all, of the world's population. Whether we know how best to use what leisure time we have today is a serious question in my own mind; how we are to educate succeeding generations so that their greater leisure will be a blessing and not a bane is something we sorely need to know.

We can all agree that nuclear warfare might well lead to universal death: suddenly for some, more slowly and perhaps even generations later, but nonetheless surely, for others. The present weapons-testing programs present a double threat: first the threat of genetic damage from radioactive fallout, and, second, the threat that continued testing may progress to actual warfare. Many people accept the weapons-testing program on the basis that it overbalances the potential harm by strengthening our military position and thus helping to insure peace.

It is a mistake to think that we can set our own pace of nuclear weapons testing. There is a snowballing effect in any such program. Our tests cause other nations to increase their pace, which in turn increases ours. It becomes a self-accelerating system: a chain reaction of a chain reaction, so to speak. Only the international control of weapons testing can stop such acceleration. Here again we sorely need social and political researches—as well as statesmanship of the highest order—to provide the basis for achieving such cooperation and control before it is too late.

Problems of this nature may well demand drastic changes in the thinking of social scientists and of statesmen alike: radically new approaches. The remarkable advances in physics over the last few years are the direct result of the ability and the willingness of physicists to think in completely new and different terms. Can the social scientists afford to do less with the very existence of mankind at stake?

Conclusion

In closing, let me urge that we cannot and must not impose upon the social scientists alone the burden of solving our social, economic, and political problems. Public issues, and especially those of world-wide portent, are the concern of all of us, and scientists of every kind (as well as nonscientists) must learn to think constructively about them, to speak out, and to make their voices heard. For this, we need to find efficient organizational patterns through which scientists may effectively express their views on vital issues. The annals, brief though they are, of such an organization as the Federation of American Scientists demonstrate, I think, that vigilant action groups of this type are indispensable in our society.

But we must not forget that the American Association for the Advancement of Science itself is constitutionally committed "to improve the effectiveness of science in the promotion of human welfare" as well as "to increase public understanding of the importance and promise of the methods of science in human progress." It is my firm hope that the Association will quickly find ways for an increasingly effective implementation of these mandates.

It has recently been said that the record of the past decade amply justifies the statement that the scientific community has a social conscience as well as a newly heightened social role. I am certain that the social consciences of the scientists will insure that they will find the way to fulfill the solemn obligations of that role.

Dedication of the New AAAS Headquarters Building

PAUL B. SEARS

Dr. Sears, the well-known ecologist and chairman of the Conservation Program at Yale University, is president of the American Association for the Advancement of Science.

THE American Association for the Advancement of Science was founded in 1848 for the purpose stated in its title. Today its rolls number some 50,000 fellows and members and more than 200 affiliated societies, representing all branches of science. After more than a century of growth and service, it is now for the first time dedicating a building well-designed and truly built for its national headquarters.

The advancement of science rests not only upon observation and experiment but upon communication as well. And it is this communication, among scientists and between scientists and the general public, that is and has been the distinctive function of the AAAS. The Association is not a builder of great laboratories, an organizer of spectacular expeditions, or a giver of generous largesse. Through its journals, its meetings, and the quiet work of its many committees, it strives unceasingly to break down barriers to understanding. These barriers, more than anything else, impede the progress of science and limit its opportunities to serve mankind.

Much has been accomplished. Much more remains to be done. Fewer than one-half of the working scientists in the United States are members of the Association. Fewer than two-tenths of 1 percent of our population are professional scientists, although the bulk of our present industrial and business activity, the professions of medicine and engineering, the practice of agriculture, and certainly national defense rest upon the findings of modern science. Also the expenditure for basic science is slight in proportion to the ultimate financial returns.

Even more serious is the widespread misunderstanding of the nature and function of science. Because the applications of science have done so much to increase material wealth, to ease the burdens upon men's brains and bodies, and to provide beguiling distractions for their increasing leisure hours, these are thought of as its chief ends. To most of us it is the handy Aladdin of the modern world and nothing more, to be summoned when wishes remain unfulfilled, or when we have stumbled blindly upon obstacles.

May I suggest, however, that the highest function of science lies not in material wish-fulfillment but in satisfying the creative instincts of the individual scientist, while giving to the generality of mankind a clearer understanding of the great natural world of which Man is inseparably a part and whose laws he may violate only at his peril. His true dignity will, in my judgement, be measured not by the power and ruthlessness he can marshal against the rest of nature but by the degree to which he respects the great processes of energy flow, material interchange and biological activity upon which rests all we have and do. These powers have been in operation for thousands of times as long as man has existed as a species.

Lest I be accused of pompous generalities, let me remind you of the increasing difficulty caused by contamination of air, water, and soil through the waste products of our industrialized life, of the growing scarcity of many raw materials, and of the unceasing pressure for living space and wholesome recreation, best measured by the rate at which the green earth is being smothered in concrete and asphalt.

That the educated man and woman of today has much to learn of even elementary science can be attested in many ways—perhaps none more vivid than the floods which last year devastated so much of New England. The bulk of the damage resulted from hazards that had been assumed without regard to the simple geology of the area or the normal pattern of stream behavior—and this by a segment of our population that is highly literate.

Not only should the average citizen have a much more realistic knowledge of science than he now gets, but the scientific training for our professions should be better rounded than it is. Lawyers and judges should be able to focus new light on ancient concepts, worked out long ago. Engineers who today wield such tremendous power ought to be better informed than they are about the great bio-

Some interior views of the new AAAS building: (top left) a corner of the library, second floor; (top right) the staff room, first floor; (bottom left) one of the editorial offices on the east side of the building, second floor; (bottom right) the board of directors' room, first floor.

logical and geologic phenomena with which they deal. Biologists should overcome their too frequent fear and ignorance of mathematics, while physicians and indeed scientists of all descriptions have much to learn from the advances of modern anthropology and other social sciences.

To bring about changes in these matters requires hard, humdrum work, largely in the field of communication. It seems to me, therefore, peculiarly fitting that this well-appointed building of the AAAS is neither a monument nor a shrine. It is, instead, a place of work. Here science will be promoted and advanced through the dissemination, interpretation, and interchange that is needed to render effective the work of discovery.

I would be remiss if I did not pay my respects to the faithful work of the executive staff, of the board of directors, and of the presidents whom I have had the honor to know and succeed in office. The Association is unusually fortunate to have as its executive officer a man whose quiet but authentic greatness we have all come to appreciate, Dael Wolfle. It is my pleasure at this time to dedicate this building to the advancement of science and to open it for inspection by this audience.

Role of Statistics in Scientific Research

JOHN W. MAYNE

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THE title of this article perhaps should read "Role of statistics in scientific experimentation," for the contribution of statistics to the design and analysis of experiments is emphasized. However, the more general title may be appropriate, for although there are many possible definitions of the term *scientific research,* all of them would probably agree that any question or problem whose answer cannot be obtained from a system of planned observations, or from a properly designed experiment, is not in the realm of science or scientific research (*1*). In any event, this article deals with the contributions of statistics to experimentation in scientific research.

If by *hypothesis* we agree to mean a trial, idea, or statement concerning the nature or behavior of a phenomenon under consideration, then the general procedure in scientific research may be stated broadly as the formulation of hypotheses and the verification of them either directly or by their consequences. The verification of the hypothesis invariably requires the collection of observations, an observation being any sort of numerical recording of information; the pattern of the observations to be collected is known as the design of the experiment (*2*).

It is well known that general principles or "natural laws," the establishment of which may be said to be the purpose of scientific research, are hypotheses that have been subjected to various tests and have become accepted. They are laws only in terms of some theory, so that in the future new "laws" may be discovered as new and more embracing theories are developed. New theories may be stimulated by new observations, by observations that are not explained on the basis of present theories, or by the new laws themselves. Thus, scientific research is circular and self-generating, involving observations, abstraction of the essential elements of observations to provide the basis for a theory, development of theory, new laws, and the prediction of new events, and then further or new observations, after which the sequence repeats itself. In this process, statistics enters at three places: (i) determining the pattern of observations to be taken; (ii) taking the observations; and (iii) comparing the observations with predictions from the theory.

Scientific Methods

Scientific research procedure may be said to consist in applying scientific methods to the study of phenomena, and an indication of the important contribution of statistics to scientific research may be readily obtained by considering the essential elements of scientific methods (*3*). These may be set forth as follows: (i) an examination of what is already known about the phenomenon of interest; (ii) the formulation of a hypothesis that may account for the phenomenon; (iii) the design of an experiment to test the hypothesis; (iv) the collection of observations or data representing the results of the experiment; (v) analyses of the data leading to the acceptance or rejection of the hypothesis.

Statistics contributes to each of the last three phases, which are sometimes grouped together and called "testing the hypothesis." In addition, every statistical test involves the formulation of an appropriate hypothesis, so it is clear that there must be a very close relationship between scientific methods and the techniques of statistics (*4*).

Experimental Variation, Random Experiments, and Random Variables

It is a common characteristic of experiments in almost all lines of research that when they are repeated the results vary from trial to trial and that

successive observations of the same or similar phenomena may vary considerably. As a simple illustration, the outcomes of successive tosses of a coin vary from toss to toss and cannot be predicted with accuracy. Small variations in the initial state of the observed phenomena which our instruments cannot detect may produce appreciable changes in the final results. For example, small differences in the initial position and motion of the gametes in animal fertilization and zygote production may result in great differences in the characteristics of the offspring. The complicated nature of the laws of certain phenomena may make exact calculations practically or even theoretically impossible. Effects of numerous small disturbing factors that cannot be controlled may result in irregular deviations from a presumed "true value."

Thus, it is impossible to calculate in advance the exact position of the results of a series of shots on a target by the same rifle even when the rifle is clamped in a vise. This is the result, in part, of the complicated nature of the ballistic laws involved and, in part, of the effects of various small disturbing factors such as wind changes, variations in bullet shape, and so forth. In all cases where one or more of these circumstances are present, an exact prediction of the outcomes of experiments becomes impossible, and irregular fluctuations and variations appear. In such cases, we shall say that we are concerned with a random experiment or a random observation, and any variable quantity that expresses the result of a given random experiment will be called a random variable. For example, a random experiment might consist of making repeated rolls of a pair of dice, of repeated drawings from a well-shuffled deck of cards, or of firing rifle shots at a target. Associated random variables would be the number of sevens obtained, the number of aces obtained, and the radial distance of the shots from the bull's-eye, respectively.

Probability

No discussion of statistics or scientific experimentation can be complete without reference to and use of the concept of probability. Unfortunately this term may be used in various ways. As employed in this paper, it does not refer to judgments but to the possible outcomes of real or conceptual random experiments. Suppose that we perform many repetitions of the same random experiment, trying to keep all relevant conditions as constant as possible (for example, we toss a coin many times). Let f denote the number of those repetitions in which a certain event A (for example, the occurrence of heads) occurs. Then f will be called the frequency of A, and if the number of repetitions is n, then f/n is the corresponding relative frequency or frequency ratio of the event A (for example, the frequency ratio of heads in 100 tosses of the coin might be 48/100 or 0.48).

If now several sequences of repetitions of the same experiment are made, several values of the frequency ratio are obtained, and it is an empirical fact that these frequency ratios for a very large class of random experiments have a remarkable tendency to stability in the sense of accumulating in the neighborhood of some fixed value. We may then postulate the existence of a number P (with $0 \leq P \leq 1$) which can be conceived as the mathematical idealization of the frequency ratio f/n in much the same way as the "true value" of some physical constant (for example, the length of a steel bar) constitutes an idealization of our empirical measurements. Such a number P will be defined as the probability of the event A in connection with the given random experiment. Owing to the tendency to stability of the frequency ratios, it is to be expected that for large values of n the observed frequency ratio f/n will be approximately equal to the probability P. The relationship between probability and random variables is shown by the following definition: A variable X is a random variable if for any given value of c a definite probability can be given to the event that X will take on a value equal to or less than c.

The Hypothesis

Formulation. The ability to formulate appropriate hypotheses demands intimate knowledge of the subject matter of the particular phenomenon concerned and the determination of the important aspects of the whole problem. It is usually a matter of intuition, experience, creative imagination, or genius, although it may also be based on analogy or the application of theory. However, the first essential is that the hypothesis be set up in such a way that it may be tested in the sense of being accepted or rejected on the basis of the information obtained from an experiment or a series of observations. Since, in most cases, the formulation of the hypothesis is necessarily in the form of some general statement that is expected to be true with respect to a whole class or population specified by the hypothesis, verification of it cannot be absolute. In practice, we can show only that the observations obtained from an experiment are consistent or inconsistent with the hypothesis within the limits of error to which the observations are subject. As Fisher has said, "an hypothesis is never proved or established, but may possibly be

disproved, in the course of experimentation" (*5*).

The rejection of a hypothesis is a more definite conclusion than the acceptance of it. In the former case, we are satisfied that we have definite reason to disbelieve the particular hypothesis stated, while in the latter case we conclude only that we do not have sufficient evidence to doubt it and realize that many other similar hypotheses might also have been accepted. Consequently, hypotheses should be formulated in such a way as to take advantage of the greater strength of the rejection statement. It is normal procedure in statistical analysis to insure that a suitable hypothesis is chosen and that it is stated unequivocally.

Proof. The proof of a hypothesis is the testing of the hypothesis. The testing in all scientific research consists in conducting an experiment and obtaining a set of observations. If the hypothesis is satisfactory in the sense of being consistent with the observations obtained, it is accepted; otherwise it is rejected. The question then arises, how can one determine whether the experimental results or observations are consistent with the hypothesis? For example, if one draws a ball from an urn 100 times each time replacing the ball and mixing up the balls in the urn, and obtains a white ball every time, he may conclude that the experimental results are not consistent with the hypothesis that the urn contains some black balls—that is, he may feel that this hypothesis is not acceptable and reject it. However the afore-mentioned results could occur if some black balls were in the urn, and if we persist in being completely positive before accepting or rejecting the hypothesis, we could never decide whether the urn contained black balls by conducting such an experiment (with only white balls appearing). Only if we are allowed to examine all the contents of the urn at the same time would we reach a decision. Similarly, if we insist on being completely positive before postulating natural or physical laws, none would ever be established as laws.

The theory and procedures of statistics enable us to reach a decision about accepting and rejecting hypotheses or laws in such a way that we know or can estimate the probability of accepting a false hypothesis or of rejecting a true hypothesis and can determine in advance what those probabilities are to be. Statistical proof by means of experimentation and testing of hypotheses is the basic form of proof (that is of reaching decisions about accepting or rejecting hypotheses) in the investigations of all sciences.

It should be noted that statistical proof is different from mathematical proof, which can be applied only within the framework of mathematics itself, and that mathematics by its very nature is incapable of proving physical or natural laws or hypotheses. In short, these hypotheses or laws cannot be tested or proved in the mathematical sense but only in the statistical sense described. In some cases, they may not be explicitly proved even in the formal statistical sense, but rather they may be accepted on the basis of accumulated evidence or experience. In such cases, the proof or testing, although statistical in nature, may be quite implicit and not involve a formal statistical test of a hypothesis. Thus it is seen that both scientific methods and statistical testing may be formal and explicit or informal and implicit. Only the formal and explicit testing of hypotheses is considered in this paper.

Testing the Hypothesis

The variations or fluctuations characteristic of random experiments introduce a degree of uncertainty into any conclusions that are drawn from the experiments. The conclusions or inferences are therefore always attended with some degree of uncertainty, as is the case with all empirical induction. Despite this, inferences can be perfectly rigorous because the nature and degree of the attendant uncertainty is itself capable of rigorous expression by means of the techniques of statistical inference, which provide a measure of the uncertainty of the inference in terms of probability. We can, for example, predict from sample results the proportion of test animals that would be killed by a known dose of some toxic agent with any desired degree of certainty (that is, at any desired probability level). No other method of measuring the uncertainty of inferences and of expressing inferences quantitatively has been successfully advanced or generally accepted. The measurement of the uncertainty of the conclusion is often more important than the conclusion itself.

The desired inferences are usually in the form of the acceptance or rejection of a hypothesis. But as a result of variability, experimental data are never exactly in agreement with the hypothesis. The problem then is to decide whether the discrepancy between experimental results should be ascribed to the variability of the data or to the fact that the hypothesis is not true. The contribution of statistics here is the rule of action known as a test of significance. This is essentially a rule for deciding in view of the experimental results whether or not to reject the hypothesis. The basic principles of such rules are (i) hypotheses that are true shall be rejected very rarely, and the probability of such rejection may be prescribed in advance by the ex-

perimenter; (ii) hypotheses that are false shall be rejected as often as possible.

As an illustration of a statistical test of a hypothesis, suppose that a large quantity called a "lot" of N units of a manufactured product is submitted for inspection and that each item may be classified on inspection as defective or nondefective. The proportion P of defectives in the lot is unknown. Let p_o be a value between 0 and 1 such that the lot will be accepted if $P \leq p_o$ and rejected if $P > p_o$. A sample of size n is drawn at random (*6*) from the lot, and on the basis of this sample a decision is to be made to accept the lot or reject it. That is, on the basis of the inspection of the sample of n items, a decision is to be made to accept the hypothesis $P \leq p_o$ or to reject it. The rule of action for a decision in this case may be stated as follows: the hypothesis that $P \leq p_o$ is rejected (that is, the lot is rejected) if and only if the proportion p of defectives in the sample of n items exceeds a suitably chosen numerical constant c (that is, if and only if $p > c$). The value of c, of course, lies somewhere in the unit interval between 0 and 1 ($0 < c < 1$) and the set of possible proportions greater than c (that is, all p such that $[c < p \leq 1]$) is defined as the critical region. The theory of statistics enables us to determine the value of the constant c so that the probabilities of accepting a "bad" lot (one with $P > p_o$) and of rejecting a "good" lot (one with $P \leq p_o$) are any desired preassigned values.

Critical Region and Types of Error

In general, a hypothesis is rejected if the sample itself or some characteristic of the sample (commonly called a sample statistic) falls in the critical region. Hence choosing a test procedure is in effect equivalent to determining a critical region, which may be defined as the region or set of values such that the hypothesis is rejected if the sample yields a value of the chosen sample statistic that falls within this region.

There are, generally speaking, infinitely many ways in which a critical region may be chosen, and they cannot all be regarded as equally good. The aforementioned basic principles represent a descriptive statement of a very rigorous principle for a proper choice of a critical region which is described briefly in subsequent paragraphs.

In accepting or rejecting a hypothesis, two kinds of errors may be committed. If the hypothesis is rejected when it is actually true, the error is said to be of the first kind (or a type I error); if we accept the hypothesis when it is false (that is, when an alternative hypothesis is true) an error of the second kind is said to be committed (type II error). Once a particular critical region has been chosen, the probability of committing a type I error and also a type II error is uniquely determined. The probability of a type I error is equal to the probability that the sample statistic will fall in the critical region on the assumption that the hypothesis is true. The probability of committing a type II error is the probability that the sample statistic will not be included in the critical region if the hypothesis is not true (that is, on the assumption that the alternative hypothesis is true).

Clearly, a critical region is more desirable than another if it has smaller probabilities of errors of both kinds. Unfortunately, it is not possible to make both these probabilities arbitrarily small for a fixed sample size. The fundamental principle for choosing critical regions prescribed by Neyman and Pearson (*7*) may be stated as follows: considering all critical regions for which the probability of a type I error is a given desired value (α), choose that one for which the probability of a type II error (β) is a minimum. The quantity α is called the size of the critical region or the level of significance of the test, and $1-\beta$ is called the power of the critical region or the power of the test. Thus, the Neyman-Pearson principle may be formulated as follows: restricting consideration to critical regions of any desired size α, choose that one which is most powerful. The test is then said to be made at the α level of significance.

The choice of size of α and β is determined by the relative importance of the errors of the first and second kinds in the particular application under consideration. For example, in the case given, a type I error would be committed if the proportion defective in the lot, P, is actually equal to or less than p_o, but the sample yields a value of $p > c$; an error of the second kind is committed if actually $P > p_o$, and the sample yields a value of $p \leq c$. Hence, if the consequences of rejecting a good lot as defective are less serious than those resulting from accepting a bad lot, then a large α and small β would be preferable to a small α and a large β.

As a further illustration, consider a slight variation of the preceding one. Suppose it is decided that it is more important that a good lot should not be rejected than that a poorer lot be accepted, so that we want the probability of rejecting a good lot to be $\alpha = 0.03$, say, and the probability of accepting a bad lot to be $\beta = 0.05$. Suppose further that by a good lot is meant one whose proportion of defective units is equal to or less than 0.10 and that a bad lot is one whose proportion of defective units is 0.15 or greater. The hypothesis may be

stated as, "the lot is acceptable." The hypothesis will be tested by drawing a random sample of size n from the lot and rejecting the hypothesis (rejecting the lot) if the proportion of defective units in the sample is equal to or greater than a certain number c $(0 < c < 1)$. In this case, the critical region will be the set of numbers in the unit interval between c and 1 (that is, all x such that $c \leq x \leq 1$), the type I error is $\alpha = 0.03$, and the type II error is $\beta = 0.05$. The problem, then, is to determine n and c such that the probability of rejecting the lot if its proportion of defective units $P \leq 0.10$ is 0.03 and the probability of accepting the lot if its proportion of defective units $P \geq 0.15$ is 0.05. If p is the proportion of defective units in the sample, we can express these conditions briefly by saying that n and c must be selected such that

$$\text{Prob } \{p \geq c \text{ if } P \leq 0.10\} = 0.03$$

and

$$\text{Prob } \{p < c \text{ if } P \geq 0.15\} = 0.05.$$

If the lot size is quite large, statistical theory shows that n will have to be 531 and that c will have to be 0.119. Thus the test of the hypothesis will consist in drawing a random sample of size 531 and rejecting the hypothesis (and the entire lot) if the proportion of defective units in the sample is greater than 0.119—that is, if the number of defective units in the sample is greater than 63.

The critical ingredient of the general theory of testing hypotheses contributed by Neyman and Pearson was the power of a test that provided a rigorous but practical basis for choosing a test procedure once the values of α and β are determined. In many statistical applications the type II error is not controlled, but it is important to realize that it exists, and the experimenter should have some idea of how large it might be. If only the type I error, α, is to be controlled, its size should be determined by the seriousness or cost of rejecting a true hypothesis. If it is important that a true hypothesis be accepted because the consequences of rejection are serious or costly, the risk α of making this error should be small. If it is important that a hypothesis be rejected if there is only a little evidence against it, α may be chosen larger. Since α may be chosen before the experiment starts and since its magnitude will partly determine whether the hypothesis is accepted or rejected, it is essential that α be fixed in advance of the experiment.

Estimation

In some cases of scientific research, the object may be to estimate population characteristics from samples of observations. The hypothesis then may be that the sample values have originated from a population that is specified in terms of certain quantities or characteristics known as parameters. The problem is to obtain the best estimates of these parameters and to provide a measure of the precision of these estimates. This can be done by determining single estimates for each parameter which are best in certain senses and by constructing confidence intervals in such a way that the probabilities that they include or cover the parameters are any preassigned values. The estimation of population parameters is often the primary purpose of scientific experimentation.

A demographer may wish to estimate the mean height of all adult males in a certain country. To do so, he selects a random sample of adult males and measures their heights. The arithmetic average of these heights (the sample mean) gives an estimate of the population mean height, M, the parameter he wishes to estimate. The sample mean in this case is called an estimator of the parameter M. In general, different kinds of estimators may be used, and some will be better in certain senses than others. The principle of maximum-likelihood estimation (*8*) leads to a very useful theory, and it may be said that for general purposes of estimation this particular theory leaves very little to be desired. For simple cases, the theory asserts that the best possible estimates of the parameters are those nonconstant values that maximize the probability of obtaining the sample.

Any estimation procedure that leads to a single value of an estimator is referred to as point estimation—thus the sample mean in the previous illustration is a point estimator of the population mean. Another widely used method of estimation makes use of the confidence interval and is known as interval estimation. This procedure applied to the example already used would result in an interval of certain length which was such that, under the assumptions involved, the degree of certainty (as measured by the probability) that the interval included the population mean would be any preassigned value. For example, the interval might turn out to be 68 ± 0.5 inches and the corresponding probability might be 0.95. Such an interval would be called a 95-percent confidence interval for the population mean.

The interval depends on the random sample drawn and is accordingly a random interval and, of course, a random variable. The proper interpretation of the confidence interval is that, if samples of the same size were repeatedly drawn at random from the population and if the random interval were computed for each sample, then 95 percent of those intervals would be expected to contain the mean height of the population. We therefore have

considerable confidence that the interval found does contain the population parameter. The measure of our confidence is 0.95 because, before the sample was drawn, 0.95 was the probability that the interval we were going to construct would contain the population mean. We are thus provided with a rigorous measure of the uncertainty of our estimate of the population mean—that is, loosely speaking, we know how "good" it is.

Estimation and the Testing of Hypotheses

A general relationship between tests of hypotheses and the estimation of parameters may be indicated by referring again to the example just cited. The hypothesis that the population mean height is equal to 69 inches could be tested at the 5-percent level of significance either by determining the critical region of size 0.05 and finding out whether the sample mean lies in this region (the critical region in this case consists of all real numbers x such that $x < 67.5$ or $x > 68.5$ inches) or by constructing the corresponding 95-percent confidence interval (67.5 to 68.5 inches). In either case, the hypothesis would be rejected (at the 5-percent level of significance). The relationship between estimation and the testing of hypotheses is not always as simple as this, but, in a broad sense, estimation may be regarded as an aspect of the testing of hypotheses. Hence, in general, the final stage in the application of scientific methods can be accomplished by means of an appropriate statistical test of significance.

Design of Experiments

Sometimes the experiment may be concerned with a number of variables under conditions which are such that all of these variables except two of interest may be held fixed at certain values, and these two may be allowed to vary. In such and similar cases, there may be no need for any great knowledge of statistics or statistical methods in order to carry out a test of a hypothesis or to reach valid conclusions concerning the relationships of the variables of interest. But in many lines of research such relatively simple situations are impossible or impractical, and recourse must be had to the more versatile and flexible methods available to the statistician. Indeed, statistical methods are becoming widely used in all fields of scientific research to insure that the experimenter obtains data relevant to his hypothesis in as economical a way as possible. The design of the experiment, therefore, is another important contribution of statistics to scientific research.

The testing of a hypothesis or the estimating of a parameter necessitates the collection of observations, but observations obtained in one way may be completely useless for these purposes, while those obtained in another way can serve these ends. Many well-known examples of this fact could be cited, and the failure of the *Literary Digest*'s attempt to forecast correctly the results of the 1936 presidential election in the United States is a well-known case in point. Hence it is clearly essential to insure that the observations obtained permit justifiable inferences to be drawn and provide a valid test of the hypothesis. It is not always easy to do this, but the statistical techniques of the design of experiments make it possible.

In designing experiments a very important role is played by "experimental errors," as the extraneous and uncontrollable variations are often called. It is usual to distinguish between two main sources of these errors. The first is the inherent variability of the experimental material, and the second is the failure or inability to standardize the experimental technique. Statistics is primarily concerned with the former, but the statistician cannot overlook the latter. He must design the experiment so as to eliminate errors of the second kind if possible, or to minimize their effect. At the same time, the experiment must provide a valid estimate of the first kind of experimental error because this estimate is needed in making the test of significance or in constructing a confidence interval. The design may be nothing more than the application of common sense, but it may also be a highly elaborate pattern based on advanced mathematical theory.

Suppose, for example, that two machines are to be tried out for use in a factory to determine which of them produces the most items a day, and that each machine is operated by one man. The experiment could be designed simply by having several men operate the machines for a few days. In order to avoid having the ability of the various workmen influence the results, it could be arranged to have a certain small number of men work one day on each of the machines. However, a further precaution should be taken, that of assigning the order in which a given man works on the two machines by a random process. In such a case where we have a two-factor (machines and men) experiment designed to control an extraneous factor (ability of the workmen) the design is called a randomized block design. The importance of the randomization should be noted because it is impossible to make statistical inferences from nonrandomized experiments. A more complicated experiment would have to be designed if the machine products (a third factor) may be made in several different sizes, if there are several different types of machines

or several machines of each type, and if a number of different men are used to operate the machines. Such an experiment is referred to as a "three-factor design with several observations per cell" and is a comparatively simple design from a theoretical point of view, although its analysis entails considerable work and rather involved computations.

An important consideration in the design of an experiment is the number of observations to be taken or the size of the sample. The advantages resulting from methods whereby valid conclusions can be drawn from a minimum number of essential observations are very important. They can represent enormous savings of time and expense, but unfortunately the advantages of such techniques have not been well appreciated or if they have the techniques themselves have not been known. The techniques vary with the problem, but all cases of the technique are concerned with the mathematics of probability. Before the minimum number of essential observations can be determined, some knowledge of the variability of the data or observations concerned is essential. No one can determine how large a sample should be, or how many observations are necessary in order to permit valid conclusions, unless the variance (*9*) of the population concerned is known or can be estimated satisfactorily. The estimate may be based on a preliminary sample or on past experience.

It is advisable to make a written draft of the proposals for any experiment (*10*). This draft should, in general, contain the following three parts: (i) a statement of the objectives or purposes and the hypotheses to be tested; (ii) a description of the experiment, including the different procedures whose effects are to be measured and compared, the measurements or observations to be provided for or collected, the types of experimental material to be used, the number of measurements or observations to be made, and so forth; and (iii) an outline of the method of analyzing the results. The last part is the one most frequently omitted, but without it the experimenter may find that the experiment has been conducted in such a way that it is impossible or very difficult to draw conclusions of interest from the results with the desired degree of confidence.

The basic principles of the design of experiments (for example, randomization, replication, block control, and so forth) have been available for some years (see *5*, *10*) so that anyone engaged in scientific research can have access to them or consult with a statistician who is acquainted with them. It is important to realize that the time to think about inferences and conclusions or to seek statistical advice is during the time the experiment is being planned and not after the results have been obtained.

Collection of Observations

If the hypothesis has been formulated correctly, and if the experiment, or pattern for collecting the data, has been properly designed, the actual collection of the data or observations is straightforward, although it may present considerable technical and practical difficulties. During this phase care must be taken to see that the experiment is carried out as designed and that whenever modifications are made that they are, as far as possible, consistent with the original plan. Statistics contributes to the collection of observations by insuring that the right observations are taken and that they are tabulated in the form most convenient for analysis and interpretation. Statistical theory also shows how the analysis should proceed when some planned observations are missing or when modifications to the original plan have to be accepted.

Use and Role of Statistics

In practice, there is a wide variation in the extent to which statistical techniques are used. At one extreme, the fact that statistical methods and analyses are tools is overlooked, and statistical calculations are substituted for common sense and scientific experience. At the other extreme, the value and role of statistics is unknown, ignored, or overlooked, with the result that much time and effort are wasted on badly designed experiments that are worthless, or else invalid conclusions are drawn from data and experiments. Invalid conclusions may be reached not only by the scientist who is unfamiliar with statistics but also by the statistical novice who fails to realize that any design and the subsequent analysis are based on certain assumptions and who fails to insure that these are satisfied or does not determine the effects of departures therefrom. It is by applying the principles of experimental design and by measuring the uncertainties of conclusions that the statistician makes his major contribution to scientific research and to other professionals in the various fields of application.

In conclusion, the role of statistics in scientific research may be set forth briefly as follows. In applying scientific methods in research, statistical theory and methodology assist in the formulation of hypotheses, provide techniques for the efficient design of experiments to test hypotheses and for the collection, tabulation, and analysis of experimental data, and enable statistical proof or tests of hypotheses to be made in such a way that the

uncertainty of the inferences can be expressed at any preassigned probability level.

Summary

Variation in experimental results is typical of almost all branches of science, and because of this variation, the problem of drawing conclusions from scientific experiments is a problem in induction. The contribution of statistical techniques to the induction problem and other phases of scientific methods is discussed in general terms. Probability, testing of hypotheses, estimation, and the design of experiments are described briefly. It is pointed out that the statistical theories of estimation and testing of hypotheses provide solutions to the problem of induction in the form of definite statements that have a known and controllable probability of being correct and that in analyzing the results of experiments the function of statistics is to measure the certainty of conclusions and to assure the scientist that only valid generalizations and inferences are made. However, in order to do so, it is necessary that experiments be set up and conducted in accordance with the principles of experimental design. The advantages of using the minimum number of essential observations and of exercising care in the collection of data are emphasized.

References and Notes

1. Mathematical research is an exception to this principle, but pure mathematics is not considered to be a science in this article, even though mathematical or abstract reasoning is a basic element of scientific research.
2. The term *experiment* as used here and throughout this article includes the various meanings aptly given in Webster's *Dictionary*: "A trial or special observation made to confirm or disprove something doubtful, esp. one under conditions determined by the experimenter; an act or operation undertaken in order to discover some unknown principle or effect, or to test, establish, or illustrate some suggested or known truth."
3. Some authors refer to "scientific method" rather than to "scientific methods"; others use the term *scientific method* to denote the combination of scientific discovery and scientific justification. On the use of the singular or plural form of the term, see J. B. Conant, *Modern Science and Modern Man* (Doubleday Anchor Books, New York, 1953) and P. F. Schmidt, *Sci. Monthly* **82**, 20 (1956).
4. It may be pertinent to quote a recent definition by R. L. Anderson and T. A. Bancroft [*Statistical Theory in Research* (McGraw-Hill, New York, 1952)]. "Statistics is the science and art of the development and application of the most effective methods of collecting, tabulating, and interpreting quantitative data in such a manner that the fallibility of conclusions and estimates may be assessed by means of inductive reasoning based on the mathematics of probability." The field of statistics is commonly divided into three aspects, descriptive statistics, statistical inference, and the design of experiments and investigations.
5. R. A. Fisher, *The Design of Experiments* (Oliver and Boyd, Edinburgh, Scotland, 1951).
6. A sample is said to be a random sample or to be drawn at random if it is selected from the population in such a way that all individuals in the population have an equal chance of being selected.
7. J. Neyman and E. S. Pearson, *Biometrika* **20A**, 175, 263 (1928).
8. A. M. Mood, *Introduction to the Theory of Statistics* (McGraw-Hill, New York, 1950), pp. 152–153.
9. The variance is a measure of the spread, dispersion, or variability of a population or sample. For a sample of size n, it is defined as $s^2 = \Sigma\ (x_i - \bar{x})^2/n$, where x_i is any sample value and $\bar{x}$ is the sample mean or arithmetic average value.
10. W. G. Cochran and G. M. Cox, *Experimental Designs* (Wiley, New York, 1950), pp. 10–14.

Spider and Web

Our cover this month shows a female of the common garden spider (*Argiope aurantia*), one of the familiar orb web spinners. It is large and brilliantly colored and is sometimes called the yellow garden spider or orange garden spider. The construction of its web is a feat of considerable engineering skill, and the web itself is an artistic creation. The photograph was made by Frederick E. Seyfarth for the Basic Science Education Series by Bertha Morris Parker, published and copyrighted by Row, Peterson and Company, Evanston, Illinois. It is reproduced here by permission of the Western Printing and Lithographing Company, Racine, Wisconsin.

According to Lillian A. Ross, associate at the Chicago Natural History Museum [*Chicago Natural History Museum Bulletin* **27**, 3 (January 1956)], spider silk is an albuminoid protein of remarkable strength and elasticity. It is produced by several types of glands and emitted through the spinnerets, which usually lie at the end of the spider's abdomen. Its tensile strength is said to be second only to that of fused quartz, and its elasticity is so great that it can be stretched one-fifth of its length. The finest single threads of silk are only about one-millionth of an inch in thickness.

Political Science of Science

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MY intention is to consider political science as a discipline and as a profession in relation to the impact of the physical and biological sciences and of engineering upon the life of man. I propose to inquire into the possible reconciliation of man's mastery over nature with freedom, the overriding goal of policy in our body politic.

In the interest of concreteness I shall have something to say about past and potential applications of science in three areas: armament, production, and evolution.

Armament

So far as armament is concerned, it is trite to acknowledge that for years we have lived in the afterglow of a mushroom cloud and in the midst of an arms race of unprecedented gravity. I shall support a proposition that may at first evoke some incredulous exclamations. The proposition is that our intellectual tools have been sufficiently sharp to enable political scientists to make an appraisal that is largely correct of the consequences for world politics of unconventional weapons.

We have correctly understood the strength of the factors that perpetuate a feeble international order even in the face of recent technoscientific developments. It is no news to us that in a divided world key decision makers expect to be better off by adopting policies that maintain the division than by paying the price of inaugurating a new system by consent. But we have not dismissed as altogether hopeless the prospects of a more perfect union, achieved by measures short of general war.

The traditional tools at our disposal for the analysis of politics prepare us to regard a voluntary relinquishment of power as much less likely than efforts to perpetuate power. On the occasions when power is relinquished, those who make the seeming sacrifice actually expect to be better off eventually in terms of power (and other values) than if they fail to pay the price. In a divided world it is not surprising to find that more elites expect to be in a better position by continuing the current system than by taking the risks involved in consenting to a new structure of power.

Yet at times voluntary confederation, federation, or integration does take place. Steps toward unity occur when elites perceive an external threat or obstacle to the preservation or fuller realization of commonly interpreted values. It occurs when elites perceive a common internal threat or obstacle. When atomic weapons appeared on the scene, it required no great acumen to see that they were introduced under circumstances in which factors of division were heavily loaded against factors favoring a new structure of unity. Nuclear weapons were introduced unilaterally by one member of a wartime coalition. It is noteworthy that the innovating member did not feel sufficiently bound to allied and associated powers to share the new weapons with the coalition. When the enemy was defeated, incentives to share were reduced. They were further reduced by the estrangement that promptly set in to separate the partners in a coalition that had been unable to achieve a high level of trust and cooperation even under the provocation of war.

It was easy to recognize that the active political elite of the largest potential rival of the United States had more inducements to procrastinate than to make the immediate sacrifices that would have been necessary for a global system of security. Moscow leaders were not faced by an ultimatum but by an inspection proposal that in the beginning would undoubtedly have laid bare the full depth of Soviet weakness and disunity. The immediate burden of sacrifice would have fallen upon one side; what was missing was a means of equalizing the cost throughout the whole course of the proposed arrangement. Moscow had no grounds for believing that the decision makers of the outside world viewed

them or their ideology or their technique with such benevolence that any visible vulnerability would remain unexploited. Certainly the annihilation of mankind seemed remote and hypothetical when compared with the deprivations that were at once likely to follow the formation of a new international structure.

It was no necessary part of the analysis to assume that leading Soviet figures loved war (after the manner of Ghenghis Khan and other nomadic "shepherds of men"). We understood that it would be political suicide for individual leaders on either side to propose full unilateral disarmament by their own government, or even to champion a ceiling or a method of arms reduction that would be regarded by colleagues or constituencies as unfavorable to continuing independence.

Nor was it part of the analysis that individuals are fully conscious of the values that influence their judgment and that the thrust of unconscious factors is toward voluntary union. So far, at least, as the upper levels of an active elite are concerned, it is implausible to suggest that continuing uncertainty will generate unconscious factors making for trust in the benevolence and good faith of alien leaders. The evidence seems to support the contrary view that protracted insecurity renders it easy to perceive the "other" as malevolent and devious. Hence the tendency is toward the perpetuation of seeming autonomy rather than in the direction of constructing a more inclusive system of public order.

We knew that the decision process of a body politic is to be understood as a complex and relatively stable network of communication and collaboration. A system acts to sustain rather than to revolutionize its own structure. This harmonizes with the fact that more acts must be repetitive than innovative if a system is stable. More specifically, structural stability is favored by the entrapment of each individual in a limited segment of the whole. The official or unofficial role of a participant determines what is available to his focus of attention, and with whom he may communicate or collaborate. The perspectives of a participant are the result of previous experience in the position and in the sequence of positions through which he has passed. The playing of any role modifies predispositions by rewarding an act with value indulgences or by imposing value deprivations. As a strategy of maintaining and improving their net value position, individuals continually make and unmake coalitions of an explicit or tacit character. Enough information has long been available to show a qualified observer that the private coalitions on which members of the Soviet or U.S. elite depend for personal advancement are coalitions whose effect is to sustain rather than to supersede a divided world. Obviously, the coalitions entered into by top officers in the Red Army, Navy and Air Force are not made with Americans, and vice versa. Nor do the corresponding coalitions of top diplomats, civil officials, or party officials cut across the intervening zone.

There are, political scientists know, typical situations in which active elite elements have expected to benefit by means of a more perfect union. Ruling families have intermarried. Today this institutional presupposition is missing; the top men of the Soviet Union or the United States are not dynastic heads. It is a frail reed indeed to rely on the hope that a Moscow-Hollywood fusion of infatuated youngsters or calculating oldsters will banish the perils of nuclear fission or fusion. Some historic unifications have been the outcome of duels between appointed champions, in the manner of David and Goliath. This institutional predisposition is also missing. No one seriously expects to stake the issue of unity on two doughty pilots armed with jets or on two teams of missile men lobbing away at one another in the South Seas. We also take note of the fact that thus far we have been unable to rely on the appearance of an external group capable of being perceived as a common threat. The U.F.O., the unidentified flying objects, have not as yet been shown to be intelligence eyes of another planet against whom we may conceivably combine.

I have been indicating how the tools of the profession provided us with categories, propositions, and cases adequate to the task of assessing the probable result of the appearance of unconventional weapons. These tools were, in fact, so used. The assertion is not being made that all members of the profession refrained from adding to the lamentations that arose as this or that circle of humanity awoke to the poignancy of the fact that the planet is moving toward apparent doom. Limited catharsis has often been obtained by railing at the stupidity or malevolence of world elites for failing to bring the current nightmare to a peaceful end. Some among us have found a measure of private satisfaction in the discovery of fresh evidence of the depravity of man and have turned for consolation to the transempirical doctrines provided by one or another of the theological and metaphysical traditions of mankind. We need not deprecate these personal solutions so long as it is clear that they are not the distinctive roles for which our profession has sought to equip its members.

To assert that political scientists had tools that enabled us to assess a major development correctly is not to say that we are complacent about our part

in the story of nuclear weapons. There is always a gap between fundamental theory and the data required to see how theoretical models explain or fail to explain specific configurations. As private scholars, political scientists did not always have access to official information; and even when he was playing an official role, a political scientist was not always shown every significant report in the possession of the government. Such limitations go beyond our responsibility as a profession. We must, however, assume responsibility for any limitation of theory or procedure that prevented us from making full use of every opportunity open to us.

I have been implying that it is possible to interpret our traditional body of theory as giving full recognition to the contextual character of politics. The classical literature made plain that specialized institutions of community-wide action are part of, and interact with, all institutions of the community, all personalities, all institutional and personal patterns in the surrounding world, and with the physical and biological environment. Modern logical technique has made it more apparent than it was formerly that the intellectual task before us is not the discovery of a small number of new fundamental categories with which to designate the context of interaction. Rather, it is apparent that all comprehensive systems are formally equivalent (hence interchangeable) at corresponding levels of abstraction (and regardless of possible differences in the number of key terms employed at each level). The inference is that within a rich intellectual tradition the most significant task is to construct a continuing institutional activity by which central theory is related continuously to events as they unfold. The fundamental categories are retained as constant features of a frame of reference elaborated and employed to delineate the contours of observational fields. The relevant context is the world political process as a whole.

The limited degree of success achieved by the profession in perfecting or in encouraging the body politic to perfect such an institutional process had adverse consequences for our role in regard to nuclear weapons. Long before atomic weapons were introduced, we were well aware of the importance of scientific knowledge for the technology of fighting. But we did not correctly anticipate the approximate timing of the impact of nuclear physics upon military technology. Although we were equipped to assess the political consequences of sudden and stupendous increases of fighting effectiveness, we did not foresee that such an emergent was imminent. Since technical developments were not explicitly anticipated, we did not clarify in advance the main policy alternatives open to decision makers in this country or elsewhere. We did not create a literature or a body of oral analysis that seriously anticipated these issues. As political scientists, we should have anticipated fully both the bomb and the significant problems of policy that came with it.

I do not want to create the impression that all would have been well if we had been better political scientists, and that we must bear upon our puny shoulders the burden of culpability for the situation of the world today. I am sure that we are not so grandiose as to magnify our role or our responsibility beyond all proportion. Yet I cannot refrain from acknowledging, as I look back, that we left the minds of our decision makers flagrantly unprepared to meet the crisis precipitated by the bomb. I have no desire to hold a kangaroo court on President Truman's momentous decision or on his principal advisers; or to give credence to the insinuation that "results" had become necessary in the face of Congressional restiveness about the cost of research and development. In the light of hindsight (that should have been foresight) I want to underline the probability that the new weapon was introduced in a manner that contributed unnecessarily to world insecurity. Perhaps the critics are right who say that the bomb should have been demonstrated on an uninhabited island before the live drops were made on Hiroshima and Nagasaki. More important is the question of how formal and effective control might have been extended beyond the decision makers of a single power. At least some members of the winning coalition might wisely have been brought into a system that operated through a common agency of inspection and direction.

Plainly there were not enough political scientists trained in physics or sufficiently aware of the implication of impending scientific developments, to do much forward thinking and planning. This points to a failure of professional recruitment and training and calls in question the then-prevailing conception of the political scientist's role. As a profession, we are concerned with aggregate processes. It is not our job to supply the working politician with what he knows already—namely, a bag of electoral and other manipulative tricks. Our distinctive perspective is not that of a trickster, although we must be familiar with the trickster's outlook and his repertory if we are to assess the causes and consequences of his way of doing business for the decision process as a whole in any context. Nor is our role limited to reiterating and celebrating the ideal aspirations of the body politic and exhibiting how value goals can be derived from fundamental postulates and principles. It is not exhausted by reporting historical sequences to be found in the rise, diffusion, and restriction of myth and technique, or even by the formalization and verification of descriptive models of a scientific

character. Part of our role, as the venerable metaphor has it, is that of scanning the horizon of the unfolding future with a view to defining in advance the probable import of what is foreseeable for the navigators of the ship of state. It is our responsibility to flagellate our minds toward creativity, toward bringing into the stream of emerging events conceptions of future strategy that, if adopted, will increase the probability that ideal aspirations will be more approximately realized.

An implication for our future relation to science and armament is that we need to develop more political scientists who have the competence to infer the weapon implications of science and technology. It then becomes possible to anticipate the implications for collective policy.

Even a moderate degree of cross-disciplinary training or continued contact should have enabled us to prepare for the advent of nuclear fission (and fusion). The *Review of Modern Physics* carried an article by Louis Turner of Princeton University in January 1940 in which 133 papers were appraised. They began with Fermi's original report of 1934 and came down to the Hahn-Strassman-Meitner researches which made explicit the import of Fermi's original experiment. In passing, it may be noted that the contributions of a dozen nations were cataloged in Turner's review. Not more than half a dozen of the 133 papers were by American authors. Perhaps American political scientists may be partially absolved for lack of foresight under these circumstances. But the over-all record of the profession is not thereby improved, for I do not find that colleagues in other countries were any more in touch than we were. Incidentally, it is worth recording that a standard college textbook in physics included a chapter in which the implications of current research were clearly spelled out. Ernest Pollard of Yale University referred in 1940 to the possibility of nuclear reactors that might generate electric power or detonate as immensely destructive bombs, or that might produce radioactive substances for research and industrial processes or for a new and frightful kind of chemical warfare. I note further that, at the time of the Fermi-Dunning experiment at the Columbia University cyclotron in early 1939, some science writers (especially of the New York *Times*) were quite definite about what was at stake.

Today, in assessing the years ahead, we need solid bases of inference about the degree to which the cost of producing unconventional weapons can be cut. Is it probable that the elites of intermediate powers will soon have at their disposal instruments capable of doing enough damage to outside powers to exert a strong deterrent effect? If so, the destiny of intermediate powers will be less grim than it has appeared to be in the recent past. If the drift toward bipolarity is reduced, there will be less hypocrisy of desperation among the elites of intermediate powers in clanging the symbols of national loyalty in public while they readjust private family, business, and political affairs in the light of the contingency that one or the other pole will dominate the world arena.

It is important to estimate the likelihood that the instruments of defense can regain ground that has been lost in recent times. In what period of time (if at all) is it probable that manned or unmanned flying objects can be harmlessly destroyed before they are on target? Is there any prospect that new knowledge may be used in a few years to seal off great areas of the globe behind impenetrable "energy" shells? (The not altogether uninviting possibility is opened that disagreeable sectors of the globe may be sealed over and left to their own devices much as small boys put dishpans over snake pits or gopher holes.)

In many ways, the most disturbing result of the laggard position of political scientists in comprehending science and technology is that we have displayed no intellectual initiative in furnishing guidance to those who are in command of modern knowledge and its instrumentalities. Alert businessmen have long been on the lookout for promising applications in the marketplace. The professional military man is now accustomed to take the initiative. The question for us as political scientists is whether we have given enough serious attention to the task of reducing the human cost of whatever violence we cannot dispense with.

As an exercise in this line of thought, I invite you to use your imagination to ask what an instrument of coercion would look like that incapacitates without killing, mutilating, or in any way imposing permanent incapacity. You and I will probably come up with the same answer: a gas or a drug or a beam that when applied will induce sleep or a similar state of suspension. We spent several billion dollars on A-and H-bombs, and it is commonly said, with some plausibility, that scientists and engineers give you what you pay for. Our suggestion (and I repeat an old proposal) is that we go down the alphabet to the P- bomb, the "paralysis bomb." The technical difficulties in the way of paralyzing a city or a region are very great, given current means of delivering a concentrated gas. Possibly the instrument can be a "P-beam," a paralyzing beam of sound or of some other kind capable of accomplishing the purpose.

Without being in the least committed to the specific devices referred to, I nevertheless assert that in the future we need not remain as passive as we have been in approaching the problem of harmonizing

considerations of humanity with the use of whatever coercion cannot be avoided.

So far as ultraweapons are concerned, it is apparent that the polar powers have reached an impasse. If they keep on they will have the capability of destroying one another several times over. Once would seem to be enough. (Resources diverted to blasting a grave assume that it is not a common one.) The polar powers have a common affirmative interest in preventing the rise of an outlying gangster or maniac who might take advantage of the declining cost of nuclear weapons to hold up or gravely damage them both. It may be that workable policy proposals will emerge from concentrated study. For instance, in return for universal inspection of new installations, the polar powers might be willing to contribute facilities and scientists to United Nations laboratories situated at intermediate points and devoted to research and development of new and fundamental scientific ideas. By providing for the possible exploitation of the results under collective auspices, it may be possible to expedite the development of the United Nations into a genuine "third factor" that concurrently expresses an inclusive interest. (Senator Anderson of the Joint Committee on Atomic Energy has already declared in favor of any policy that holds promise of joint activities that "obsolesce" old weapons around which vested and sentimental interests are crystallized.)

Since we are aware of the unforeseeable timing of the many factors that may affect a resort to arms, it is evident that rational policies on behalf of peaceful cooperation do not rely on a single avenue of approach. Wise strategy appeals to as many potential pockets of motivation as possible by making continual use of tactical ingenuity in applying every instrument of policy (diplomatic, military, economic, ideological).

It is generally believed that peaceful cooperation can be most readily encouraged in the field of economic growth. And there are grounds for predicting that developments that impend in the technology of production will rival the leaps that have recently occurred in weapon technology. This brings me to the consideration of our relationship as political scientists to these potentialities. Although the devices that contribute to production may also be employed for destructive purposes, my present concern is with the affirmative uses of science.

Production

Impending is control of weather and climate. As our knowledge of the upper atmosphere increases, it will be obvious that the seeding of local cloud formations is a relatively trivial precursor of hemispheric or global control. Impending is the solution of the problem of obtaining pure water at low cost from the sea for irrigation purposes. Taken in conjunction with newly available energy sources, it is not too early to anticipate the reclamation of the wastelands of the earth—the deserts, the polar ice caps, the tropical rain forests, the mountains (leveled to productive plateaus). As J. G. Harrar has told us, the total solar energy that reaches the earth every 48 hours is approximately equal to all the known reserves of coal, petroleum, and wood. Clearly the conversion of relatively minute amounts to usable form would meet the energy needs of future generations. Already enough progress has been made to indicate that in the immediate future many local power needs can be supplied more economically by solar energy than by nuclear installations.

As political scientists, we are conscious of the implications if great resource changes are introduced into a divided world. Imagine that the arid areas inside the Soviet zone are populated with the density of the fertile districts. Suppose further that the non-Soviet world does away with the arid lands of the United States, Mexico and South America, the Sahara, and the Middle East. If these regions become as populous as the more habitable parts of the countries where they are located, the population of the non-Soviet world will be increased relatively more than the Soviet Area.

Think next of the tropical forests. If the tropical rain forests of central and northern South America, of mid-Africa, of India, and of Southeast Asia are made fit for human life to the same degree as the more temperate regions near them, the relative population of the non-Soviet world will appreciably increase.

If mountainous areas are transformed into plateaus, and the plateaus are populated to the density of neighboring areas, the population of the non-Soviet world will also sustain a relative rise.

We know that the political consequences of changes in population and energy production depend on the impact of these developments on the "threat value" or the "asset value" of the members of the world arena to one another. We expect that the flow of capital and know-how required to modernize production tends to conform the facts of economic growth to the configurations that predominate in the arena of world politics. One alternative of policy is to mitigate or modify this tendency to pour the concrete of capital investment into the mold of a current power alignment. To what extent can this result be achieved by instituting multilateral control of great programs of reclamation in selected districts? Can the Sahara, for instance, be jointly developed? Since the ruling

circles of a split world pursue different objectives in terms of social structure and ideology, it is only feasible to think of even restricted programs of multilateral cooperation within the frame of an agreement in which are prescribed the permissible proportions between governmentalized and non-governmentalized operation to be preserved at successive stages of the project. Further, it will be essential to determine whether the program is intended to consolidate an existing national unity or to lay the foundation for a new nation (one drawn, for example, from widely varying ethnic sources, or from a single principal source of people and culture).

The factor of geographic dispersion has an important bearing on our expectations. The sources of solar and atomic energy are more capable of being widely distributed than the sources exploited by a technology of fossil fuels like petroleum and coal. It is axiomatic that a decentralized pattern of access to energy provides a favorable resource base for a decentralized outlook and that the perspectives comprising such an outlook sustain a decentralized network of policy formation and execution. If a reversal is to occur in the trend toward bipolarizing the world arena and a pattern resembling the Great Power System of the last century is to revive, two conditions at least must be fulfilled: (i) a network of strong, coordinate centers of energy production and (ii) cheaper costs of producing the newer weapons.

I have been talking of the resources found in the wastelands or neglected opportunities on the surface of the continental blocs. We must take the fact into account that new resources are in prospect whether we look beneath or above the land surface of the earth. To begin with the seas: we shall learn to mine the waters for minerals and to farm the oceans for foodstuffs on a scale hitherto unthinkable. With regard to resources above the surface: we are close to the first experimental expeditions to the moon and, presently, the planets. In this setting, the traditional questions that center around the control of air space take on new significance.

As specialists on public law, it is not difficult for us to anticipate the form in which conflicting claims to these new resources are likely to be phrased. In connection with the seas, those who push particular claims to the exclusive exploitation of a given region will talk in terms of "the territorial sea," "contiguous zones," "jurisdiction," and the "continental shelf." Nation states whose officials push particular claims to share in exploiting the resources of an area will invoke the "freedom of the seas" and other internationalizing concepts. The probing of the upper atmosphere, satellite launching, space platforms, and the like will pose the problem of how to adjust claims to exclusive control of "air space" against claims to share control. As expeditions to the moon or the planets become more imminent, the question of "who owns what" or "who controls what" will bring into the debate the authoritative language traditionally employed in connection with the acquisition of territory ("exploration," "occupation," "conquest," and other concepts emphasizing priority in time and effectiveness of control).

As clarifiers of the goals and alternatives implicit in a decision process, and as advisers of the participants, we have an opportunity to reduce the amount of unnecessary friction by establishing a frame of reference in advance of the facts. When factual details appear, they will of course exhibit some novel elements; common goals and principles will not. The members of the world community have a long history of accommodating "exclusive" claims and "sharing" claims with one another (as new resources provide new base values for the participants in the world arena).

It is, of course, essential that in taking advantage of this opportunity we deal with the entire context of value goals and principles as they relate to potential facts. I have referred to sets of doctrines that in all probability will be invoked when claims are made. The chief function of these formulations is to guide the attention of decision makers to the context in which pertinent activities occur. Formulas assist in recognizing and evaluating the consequences for international public order of accepting the exclusive or the sharing claim in particular cases or categories of cases.

When we examine past trends in the world arena, it is not difficult to recognize that shifts have occurred in the relative emphasis laid upon exclusive or sharing claims. Grotius was speaking for the Netherlands and for other challengers of the claims of Spain, Portugal, and England to monopolize great stretches of the seas. Sweeping readjustments were made in doctrine and in applications relating to the seas. At first they were mainly in the direction of consolidating an international order in which sharing claims were widely accepted ("freedom of the seas"). In recent decades the trend has been the other way. As my colleague Myres S. McDougal has shown in some detail, claims to the exclusive enjoyment of resources have been accepted as "reasonable" in the light of facts that have appeared in the course of applying science and technology. A recent tabulation shows that no more than 13 states accept the ancient 3-mile rule for the territorial sea. Forty-five states repudiate it in varying degree, claiming wider limits. Contiguous zones of many kinds have been accepted (for the administration of customs, the security of neutral states

against belligerent activities, fishing conservation, appropriation of the resources of the sea bed and of the continental shelf).

When we think configuratively about the problems raised in reference to the new resources, it is clear that instead of blanket principles (like "freedom of the seas" or "freedom of the air") the most fruitful policy alternatives are likely to emerge when we anticipate the appearance of characteristic factual contexts and consider how the values chiefly at stake in them can be maximized. Hence we would not expect to apply the same prescription to (i) the sharing of air space for weather observation (where equipment is used that is expressly designed for the purpose and perhaps registered, and when the information obtained is made public) and (ii) to the sharing of air space for projects of weather or climate control that may be deleterious to local values.

The contextual (or, synonymously, the configurative) approach is a challenge to imagine the full range of possible means of anticipating and resolving difficulties. On matters concerning which the greatest uncertainties appear, it is appropriate to call attention to the need of exploring the possibilities of agreement in advance of conflict. The inference is that no time should be lost, for instance, in putting into the hands of the United Nations the facilities for research, development, and operation of satellites, "space platforms," and travel beyond the limits of the earth's atmospheric and gravitational fields. Doubtless the United States and the Soviet Union will continue to compete with independent programs. Since the polar powers have a stake in moderating the conflict in which they are engaged in the hope of eventual harmony through agreement, not catastrophe, a practical method would appear to be to strengthen the "third factor" (especially when both powers are also included within it).

The rapid introduction of new resources under present conditions calls for some degree of community and regional planning, and planning poses thorny questions about the structure and ideology of society. To an increasing extent, questions of this kind need to be answered directly rather than by default. It must be conceded that American political scientists are not especially well equipped to participate in the planning function on the scale required. Although we are accustomed to corroborate the classical authorities and the Founding Fathers in praise of the middle classes as indispensable to popular institutions of government, we have not as a rule dealt with these traditional doctrines in significant ways. For instance, we have not explored the principles of proportion that are most likely to consolidate or to sustain at various stages of industrial growth the perspectives and operational technique of popular government. Shall we, for example, rely upon a 30–40–50 rule to guide public policy in regard to the permissible degree of market control permitted to private interests? (For example: When one interest has 30-percent control of output, shall it be subject to special regulations designed to nullify the side effects of power that go along with economic control? When one interest rises to 40 percent, shall we put governmentally appointed trustees on the board of directors? At 50 percent, shall government trustees predominate?)

Whatever the workable rules of proportion may be in representative contexts, it is evident that we need to guide our studies of trend correlation and of comparative cases in order to improve the available bases of inference in such matters.

The same approach—the search for rules of proportion—applies to every institutional and personality pattern in a body politic. What are the optimum proportions of community resources to devote to elementary, intermediate, advanced, and ultra-advanced education? to research and development in science and technology? to positive and negative sanctions for correctional and other purposes?

One way to jar "cakes of custom" out of the mind is to draft specifications for the first Mayflower expedition to establish continuing occupation outside the earth. (Possibly it could be "Noah's jet"?) What proportion of men, women, and children of which culture or combination of earth cultures shall we select? What ideological traditions, secular and sacred? What class backgrounds (elite, mid-elite, mass)? What individual group interests? What personality structures?

By asking questions of this kind, we are in a position to assess our present stock of knowledge concerning the interdependence of institutions specialized to power and all other institutions in the social process of any community, together with the forms of personality involved. These, of course, are the recurring issues of political science and historical interpretation as well as policy.

Evolution

I have been referring to a few implications of science and technology for weapons and production, and sketching some political ramifications. As political scientists, we are perhaps even less well prepared to anticipate developments in genetics, experimental embryology, and related disciplines. Taken together, these fields signify that, as Julian Huxley has often put it, man is on the threshold of

taking evolution into his own hands. By influencing the genes that constitute the key units in man's biological inheritance, we affect the entire potential of future generations.

Important as recent innovations are in radioactivity, I do not want to give more than passing notice to the dangers that they embody. The only feasible means of coping with these factors is by policies that avert war and preparation for war and install proper precautions in the handling of high-energy radiation for other purposes.

Quite recently the dangers that arise from radioactivity have been authoritatively brought to public notice: all high-energy radiation that reaches the gonads stimulates gene mutations: more than 99 percent of all mutations are dangerous; genes can only be eliminated by the death of the gene carrier or by his incapacity to reproduce. Nearly 2 years ago H. J. Muller told us that the bomb tests since the war had already exposed the inhabitants of the earth to radioactivity comparable with that of the inhabitants of Hiroshima and Nagasaki after the original explosions. He estimated that about 80,000 harmful mutations are involved and that "it will mean, in the end, several times this number of hampered lives."

It has been pointed out that perhaps the most satisfactory index of genetic damage is the sum of tangible defects existing among living individuals. We are speaking of such stigmata as "mental defects, epilepsy, congenital malformations, neuromuscular defects, hematological and endocrine defects, defects in vision or hearing, cutaneous and skeletal defects, or defects in the gastrointestinal or genitourinary tracts." We are informed that about 2 percent of the live births in the United States have defects of "simple genetic origin and appear prior to sexual maturity." If mankind were subjected to a "double dosing" of radiation, the present level of genetic defects would rise and would eventually be doubled.

Regulatory measures are obviously needed against wars and weapon tests, and they are essential to the disposition of nuclear waste from industrial plants. (It has been remarked that a nuclear power plant is to be viewed as a large scale production of both highly poisonous gas and explosives under a single roof.)

The principal questions to which I desire to call attention poses issues of a relatively new and different order. Some of these questions have already come up in controversies over artificial insemination. They have embarrassed the champions of the orthodox prescriptions that prevail in several fields (theology, ethics, and jurisprudence). Shall we call a child legitimate whose biological father is not identical with the sociological father? even with the consent of the latter? with spermatazoa from a known or unknown source? (A possible international question is whether a nation state like the United States can claim the child as a citizen if the spermatozoa employed originated with an American mail order house and was sent by airmail for use abroad.)

Poignant as these issues are in specific cases, they do not confront us with the consequences for public order that are often anticipated if the progress of biology separates insemination and child bearing from genital contact. The assumption is often made that the continuation of sexual rectitude and even civic order depends on charging every genital contact with the blessings and perils of procreation. The impending improvement of oral contraceptives, joined with other recent advances, is a factor that already suggests the wisdom of other norms and sanctions of public order.

Other developments are threatening current ratios of the influence and power of the sexes. Given the millions and millions of spermatozoa produced by one male and the technique of canning by refrigeration, any very large number of males becomes relatively redundant as an aid to procreation. Must the male rest his future on other values such as the strictly esthetic appeal of the male contour? Before the female of the species becomes too complacent in this context, it may be worth recalling the significance of some current experiments for the removal of the primordial female function from the body and into other receptacles. (Women, too, may have to rely upon their charm, a role for which their experience has provided extensive preparation.)

Apparently we are closer than most of us like to think to the production of species that occupy an intermediate position between man and the lower animals (or even plants). It is sometimes said, even in august quarters, that "one has not yet succeeded in making a species from another species." Theodosius Dobzhansky notes, however, that "the feat of obtaining a new species was accomplished more than a quarter of a century ago." In recent decades, a fair number of new species have been brought into being. It is also true that some species that exist naturally have been recreated experimentally. A garrison police regime fully cognizant of science and technology can, in all probability, eventually aspire to biologize the class and caste system by selective breeding and training. Such beings can, in effect, be sown and harvested for specialized garrison-police services or for other chosen operations.

Great strides have been taken in brain design.

Experimental models of robots have been built that solve problems of a rather complex order in a given environment. Some of these machines look after themselves to a degree, obtaining and using the raw materials required for energy and repairs. Already it is claimed that the function of reproducing its kind, and of interacting with others, can be in-built.

The question then rises: Given our concern for human dignity, when do we wisely extend all or part of the Universal Declaration of Human Rights to these forms? When do we accept the humanoids —the species intermediate between lower species and man, and which may resemble us in physique as well as in the possession of an approximately equivalent central nervous and cortical system— as at least participants in the body politic? And at what point do we accept the incorporation of relatively self-perpetuating and mutually influencing "supermachines" or "exrobots" as entitled to the policies expressed in the Universal Declaration?

It is obvious that we are not very well-equipped by cultural tradition to cope with these problems. A trait of our civilization is the intense sentimentalization of superficial differences in the visible format of the groupings to be found even within the human species. Recall the theologians, ethicists, and jurists who have devoted themselves to the elaboration of symbols to show that the white race alone is genuinely human and hence solely entitled to the dignity of freedom. Recall, too, the counter-assertions, nourished in the soil of humiliation, that have arisen among ethnic groups who seek to overcome their contempt for themselves by dragging down the pretentions of the white imperialist.

Let us recognize that the traditions of certain non-Western European civilizations are in some ways better adapted to the problem than the Greco-Roman and Judeo-Christian perspectives. They possess a relatively broad basis for identifying the primary ego of the individual with a self that includes more than strictly human species in the congregation of living forms. A world view that includes the possibility of reincarnation in lower animal shapes, for example, may prepare its devotees to empathize more readily with other than strictly human species and varieties. (Even they, however, may have their troubles with a mobile power plant in nearly human form.)

The most disturbing question, perhaps, arises when we reflect upon the possibility that supergifted men, or even new species possessing superior talent, will emerge as a result of research and development by geneticists, embryologists, or machine makers. In principle, it is not very difficult to imagine a superior form. For instance, our sensory equipment does not enable us to take note of dangerous radiation levels in the environment. We have no inborn chattering of a Geiger counter.

I have mentioned taking the intellectual initiative for the use of science and technology for the fuller realization of our value goals. It is plain that, if we bring certain kinds of living forms into the world, we may be introducing a biological elite capable of treating us in the manner in which imperial powers have so often treated the weak. A question is whether the cultivation of superior qualities ought to be limited to intellectual capability. The answer, I feel confident you will agree, is in the negative. We need to be sufficiently vigilant to prevent the turning loose on the world of a hyperintelligent species driven by an instinctual system especially inclined toward predation. The blood-stained story of our own species is only too familiar (the stories about succulent missionaries whose bodies were more readily incorporated than their messages are not wholly without foundation). Can we improve the prospects of developing a form of intelligent life copied, not after our own image, but after the image of our nobler aspirations?

It is not to be overlooked that the problem of human capability can become acute if in the years ahead we escape from our present habitat on the earth or are visited by other forms of intelligent life. There are, after all, untold millions of environments resembling that of our solar system, and it would be more remarkable to find that but one planet is inhabited by a complex living form than to encounter parallel developments. It would of course be embarrasing, at least, to discover that we are the savages or that we are put together on a markedly inferior biological plan.

Our Future Program

The fact is that many of the problems to which I have been referring will be upon us long before we can make great changes in the ideological outlook or the sociopolitical patterns of life in this country or elsewhere. The same point applies to ourselves in our role as individuals and as members of the political science profession. Considering our present predispositions, how can we improve the likelihood of contributing to the decision process at every level, from the neighborhood to the world as a whole?

It is abundantly clear that the impact of science and technology does not occur in a social vacuum but in a context of human identifications, demands, and expectations. I make the modest proposal that it is appropriate for political scientists, in company with other scientists and scholars dealing with hu-

man affairs, to improve our procedures of continuous deliberation on the potential impacts of science and technology upon human affairs. No doubt the American Political Science Association and other professional societies constitute an appropriate network for the purpose. We can sustain continuing conferences devoted to the examination of emerging developments. As fellow professionals, we have special responsibility for giving thought to the aggregate effects of any specific innovation.

One professional contribution, it appears, is to project a comprehensive image of the future for the purpose of indicating how our overriding goal values are likely to be affected (if current policies continue).

A closely related contribution consists in clarifying the fundamental goal values of the body politic. We are accustomed to confront political ideologies with new factual contingencies and to suggest appropriate specific interpretations. We also confront political doctrines with rival doctrines and with comprehensive theological and metaphysical systems. I have called attention to the point that the basic value systems of European civilization, in particular, are likely to be exposed to sweeping challenge as biology and engineering narrow the obvious differences between man and neighboring species and between man and centrally operating machines. The crisis will be peculiarly sharp if we create or discover forms of life superior to man in intellect or instinctual predispositions. Our traditions have not been life-centered, but man-centered. We possess various paranoidlike traditions of being "chosen." Clearly a difficult task of modifying these egocentric perspectives lies ahead.

The third task is historical and scientific. It is historical in the sense that by mobilizing knowledge about the past we are enabled to recognize the appearance of new patterns and the diffusion or restriction of the old. It is scientific in the sense that we summarize the past in order to confirm (or disconfirm) propositions about the interplay of predisposition and environment. If we are to serve the aims of historic recognition and of scientific analysis, one of our professional responsibilities is to expedite the development of more perfect institutions specialized to continual self-observation on a global scale. Self-observation requires guidance by a system of theoretical models of the political process in which a continuing gradation is maintained between the most inclusive model and submodels adjusted to more limited contexts in time and space. Continual self-observation renders it necessary at each step through time to reevaluate the appropriateness of the operational indices for the variables and concepts employed at the most recent step. In this way, all the concepts that figure in systematic, descriptive political science can be kept chronically pertinent to the ordering of political events as the future unfolds.

The fourth task is inventive and evaluative. It consists in originating policy alternatives by means of which goal values can be maximized. In estimating the likely occurrence of an event (or event category), it is essential to take into account the historical trends and the scientifically ascertained predispositions in the world arena or any pertinent part thereof.

The relationship of American political science to these tasks is in many ways unique. The typical department is a microcosm of the macrocosm of university faculties of the social sciences and humanities, and the school of law. It is no secret that a syndicate of philosophers, historians, behavioral scientists, and public lawyers is capable of producing some degree of tension among themselves, especially when budgets are at stake. This has led to the suggestion that every component skill should be sent back where it came from—to the departments of philosophy, history, sociology and psychology, for example, and to the law school. In this way, political science could be given back to the Indians. The catch is that we are not agreed who the Indians may be.

The present situation does make it possible for political scientists to take the lead in integrating rather than dividing our intellectual community. Compared with an entire university, which has become a noncommunicating aggregate of experts, each department of political science can be a true center of integration where normative and descriptive frames of reference are simultaneously and continuously applied to the consideration of the policy issues confronting the body politic as a whole over the near, middle, and distant ranges of time.

The profession is advantageously situated, therefore, to take the lead in a configurative approach to the decision process in society. Where it plays this part, political science is the policy science, *par excellence*. If the implications of science and technology are to be correctly appraised, it will be essential to recruit some trained personnel from such fields into political science, to improve the science content of professional education, and to provide for continuing cooperation among the professions involved.

It is quite unnecessary for any one individual to emulate the universal ambitions of Renaissance man. But if we are to take the lead in performing the configurative or matrix function, it is quite essential for the profession as a whole to achieve the division of labor, the understanding, and the

insight capable of realizing as fully as possible the dream of relevant universality. Each of us can at least widen the boundaries of the self and open the way to identify with living forms that differ from traditional images. We can step toward the possible reconciliation of a growing mastery over nature with the dignity of freedom for all that lives. In the congregation of living forms, human life may come to play a yet more distinguished role in generations to come, a role that transcends even the vision of the commonwealth of man championed by the distinguished political scientist and the statesman the centennial of whose birth we take pride in according special commemoration (*1*).

Note

1. Many of the papers at the annual meeting of the American Political Science Association were devoted to the life and thought of Woodrow Wilson.

BOOK REVIEWS

The Exploration of Mars. Willy Ley and Wernher von Braun. 16 paintings in color and 5 in black and white by Chesley Bonestell. Viking, New York, 1956. $4.95.

The collaboration of Ley and von Braun has joined the important and colorful popularizer of rocketry and space travel with von Braun, the most unique figure to arise from the work at Peenemunde in the creation of the V-2 rocket. Von Braun is now an American citizen carrying out research for the American government on high-altitude rocketry. He wants to start on construction of the Mars project now—the only thing lacking is the money. These two men have created the most realistic and powerfully written description of a trip to Mars that I have ever read. There is only one assumption required to make this trip real now, and that is a manned space station orbiting the earth.

The first part of the book gives a very intriguing description of the known observational astronomy of the planet Mars. Careful and critical evaluations of Mars with regard to atmosphere, water, and plant life are given. The conclusion is that on Mars probably lies the evidence that life is not confined to earth in our solar system. Most of us are romantic enough to go to the next step in the thesis and postulate some form of animal life also!

The major portion of the book deals with a careful description of what is required on an exploratory trip to Mars in material, men, fuel requirements, best orbits, and other details that can make or break any expedition. The essential requirements for a landing on Mars and the return trip are given. The whole development of the concept is so realistic that one feels as if he is reading accounts of an already accomplished mission.

The illustrations by Bonestell are beautiful and, without meaning to be trite, out-of-this-world. I am certain that whenever an expedition reaches Mars that color photographs of the surface conditions will not be more fascinating, but just more authentic. The description of the planet's surface conditions by the authors are followed by the illustrations exactly. In the back of the book are six pages of tables, including essential data required by a Mars expedition. Because of the present limitations of chemical fuels, a manned space station is required for this trip. Our present earth satellite program is the first step, and it should be followed by the manned space station. At that time, what are today's speculations become tomorrow's facts.

THOMAS S. GARDNER
Hoffmann-La Roche, Inc.

Earth, Sky and Sea. Auguste Piccard. Translated by Christina Stead. Oxford Univ. Press, New York, 1956. 192 pp. Illus. + plates. $4. (Also published as *In Balloon and Bathyscaphe,* Cassell, London, 1956).

This is a translation of a manuscript published under the title *In Balloon and Bathyscaphe* by Cassell and Company, Ltd. It is the Swiss author's personal account of his production, launching, and initial use of a manned free balloon in the stratosphere and of the development of the bathyscaphe for abyssal oceanic explorations.

The author describes in semitechnical language how he came to choose specific materials and design in the production of the cabins for the balloon basket and for the two bathyscaphes. He describes actual problems of design that were discovered and corrected after field trial or the use of models and gives justification for theoretical considerations in the construction of cabins and floats.

The story is told by development of the various problems as they arose. The emphasis is on the

careful consideration of safety factors and the full use of scientific developments in providing for success by anticipating the problems that might arise. It becomes obvious to the reader that here is a dedicated scientist whose lifework is being realized because of his careful plans and his patient and thorough work on the problems.

The book is well illustrated both with diagrams that aid in understanding solutions to the problems and with photographs of the operating equipment.

Two bathyscaphes were produced, one under Belgian auspices and the second under joint Swiss and Italian sponsorship. The author, after several preliminary dives, went with his son to a depth of 1732 fathoms off the Island of Ponza, about 60 miles northwest of Castellammare, in September 1953.

Appendixes are included giving some of the more technical details of the construction of portholes, magnetic and other valves, electromagnets for control of ballasting, floats, keels, doors, and special gages, as well as details of methods for testing metal strength and of provisions for passage of tubes and wires.

A discerning view of the future is given with respect to research in basic design of oceanic transport and in general oceanography.

This is an entertaining and informative book with a solid and respectable treatment of an unusual subject.

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Diseases of the Endocrine Glands. Louis J. Soffer. Lea and Febiger, Philadelphia, ed. 2, 1956. 1032 pp. Illus. $16.50.

This is a thorough revision, and *not* a "scissors and paste" edition of the 1951 copy. Louis J. Soffer, the author of this book, together with his contributors, J. Lester Gabrilove and Arthur R. Sohval, has thoroughly revised the book, and in so doing has taken advantage of the advances made in the field of endocrinology during the past 5 years. In addition to revising this book, Soffer, Gabrilove, and Sohval have included new information on aldosterone, the synthesis of the polypeptides—oxytocin and vasopressin—the chemical identification of corticotropin, the identification of triiodothyronine, and the clinical use of prednisone, prednisolone, and the halogenated cortisols.

The present volume is much more than the title implies: it is a book heavily laden with information pertaining to the normal endocrinology as well as to the diseases of the endocrines. Furthermore, the book provides a very accurate account of the embryology, anatomy (gross and microscopic), and physiology of each endocrine gland *before* the diseases are discussed. In this regard, it gives a full account of the career of each gland, including the pathological variants that may disturb endocrine function.

Diseases of the Endocrine Glands is divided into five main sections: (i) "The hypophysis," (ii) "The adrenals," (iii) "The gonads," (iv) "The thyroid," and (v) "The thymus and the parathyroids." The authors have omitted judiciously their previously included sections on carbohydrate metabolism and diabetes mellitus. Since this field merits separate attention, and since it is divided among numerous specialties, I wholeheartedly feel that Soffer, Gabrilove, and Sohval were very astute in utilizing the precious page-space previously used for diabetes mellitus for the greater development of the aforementioned five sections.

Of particular interest to me were the sections on the gonads by Arthur R. Sohval. This is by far the most extensive treatment the subject has ever received.

All the sections throughout the book are articulate and highly documented. Each section ends with an exhaustive bibliography of references that have been collected and cited judiciously.

The textual statements are correct, interestingly presented, and crystallize thought that was previously scattered in numerous scientific papers and monographs. No student, research worker, or physician in this field or in general medicine should be denied the opportunity of reading this book. I can recommend this book without reservation because of its accuracy, clarity, cohesion, and above all its scholarly contribution to endocrinology and medicine.

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Stratigraphical Palaeontology. A study of ancient life-provinces. E. Neaverson. Clarendon Press, Oxford, ed. 2, 1955. xii + 806 pp. Plates + illus. £5, 5s.

The first edition appeared in 1928, and since that time much new information has quite literally been dug out of the rocks, so that the only recognizable remains of that edition are the illustrations. E. Neaverson has "revised and enlarged" to good purpose.

The first two chapters explain the preservation and distribution of fossils and their use as horizon guides. Attention is called to faunal relationships

and environmental differences. It should also be noted that plants, as well as animals, are recognized. In the discussion of the use of fossils as horizon guides, the usable factors from protozoans to mammals are handled in some detail, but plants get a short page! The next 15 chapters discuss the various faunas, period by period, from Cambrian to Neozoic mammalian faunas. Here the emphasis is, as one might expect, primarily on the British area, however, the rest of the world is included, but with less detail. Fossil flora occupy only one chapter, but they do turn up occasionally among the fauna. The last chapter is a careful discussion on the relations of the continents and oceans, and Neaverson reaches the conclusion that broad land bridges and continental drift are not supported by paleontological information. He reasons from tetrapods and plants for the continents and from faunal distribution for the oceans and makes a good case.

In the 27 years since the first edition appeared, our faunal lists have grown, and many more areas of the world have been explored geologically. This wealth of information has been drawn upon in the revision. The text figures and plates are excellent. The sketch-maps are, as the term implies, sketchy, on different scales and with very little detail. My reaction is that they should all be the same scale, even if the larger areas had to be folded. Of course, they have to be generalized, and it is a minor point.

This is a thought-provoking book and one that would be profitable to the geologist as well as the paleontologist. My congratulations to Neaverson on his revision and enlargement and to the Clarendon Press.

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The Human Heredity Handbook. Amram Scheinfeld. Lippincott, Philadelphia, 1956. 276 pp. Illus. $3.95.

Amram Scheinfeld has established a reputation as a successful popular writer on human heredity through his previous books *You and Heredity* (1939) and *The New You and Heredity* (1950). *The Human Heredity Handbook* covers the same range of material as the former publications but is in more concise form. The author states that "the Handbook has been planned by my publishers and myself to serve a new and growing need for a quick, handy, untechnical, practical (and economical) guide to the general subject of human inheritance, and to provide direct, uncomplicated answers to such specific questions on the subject as may be asked by parents and prospective parents, teachers, students, medical men, marriage counselors, and other interested persons."

The book includes brief explanations of the principles of heredity, sex determination, and multiple births. This is followed by chapters on the inheritance of external features, physical and mental defects, exceptional abilities, and behavioral traits. Current theories on human evolution and the origin and significance of the races of man are discussed. The text concludes with medicolegal problems, counseling, and a consideration of efforts to improve the human species through eugenic practices. In general, the discussions are scientifically accurate and, although the professional may wince at certain passages, the difficulties are those inherent in giving "uncomplicated answers" in popular language to complicated problems.

The appendix is a welcome and potentially useful addition. The reader is provided with a brief glossary of some technical terms dealing with human heredity and a list of references for further reading in books and periodicals. To those seeking professional advice and information on problems of human heredity, the names and addresses of leading institutions in the field are given. Curt Stern's laboratory at the University of California is left out of the list—an unfortunate omission.

The book is illustrated by simple line drawings and diagrams by the author. About half of them are new, and the rest are taken directly or modified from illustrations in *The New You and Heredity*. Some are helpful, as the more detailed diagram of sex-linked inheritance; others are dramatically explicit, as the representation of human stature variations. Of doubtful use are the cartoon sketches intended to aid in comprehension of different kinds of gene action as "busy body" (manifold effects) and "Rip van Winkle" (late onset); or the peculiarly inappropriate pictorial representation of the "jig saw" theory of ancestory.

The author is perhaps overly optimistic in considering that his readers may wish to own the *Handbook* for quick reference as well as *The New You and Heredity* for more detail. (Physicians called upon to give genetic counseling, for example, may find Sheldon Reed's *Counseling in Medical Genetics* more useful for detailed information in that particular field.) Nevertheless, Amram Scheinfeld and his publishers are undoubtedly correct that there is a large audience for a book of this kind on human heredity, and the public will be the wiser for reading it.

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Surgical Treatment of Penetrating Wounds of the Heart, Pericardium and Mediastinum. N. I. Grigor'ev. State Publishing House of Medical Literature, Moscow, 1953. 136 pp. Illus. (In Russian.)

The first surgical attempt at removing a bullet from the heart of a wounded patient is credited in Russian medical literature to a surgeon, A. G. Podrez, who performed this operation in 1897. Podrez' technique consisted of placing two sutures on the heart wall and then making an incision between these sutures at the site where the bullet was suspected of being lodged. Podrez reasoned that, after the removal of the bullet, tightening of the sutures would stop the hemorrhage and close the wound.

The operation was technically not successful (the bullet could not be found following ten separate needle punctures), but the patient did survive. The feasibility of surgical intervention was thus established. Moreover, the idea of placing sutures before making the incision required for extracting the foreign body from the heart prepared the basis for the development of modern techniques in this field of surgery. Utilizing this method, a Russian surgeon, V. G. Manteĭfel', successfully removed a bullet from the right ventricle of a patient in 1905.

During World War I a number of reports appeared regarding surgical removal of foreign bodies from the heart and pericardium. These were primarily individual case reports. Careful studies, with indications for and against surgical intervention, began to appear during World War II. The studies of two Soviet surgeons, P. A. Kupryanov and I. S. Kolesnikov, published in 1946, are of particular importance. They observed 122 patients with penetrating cardiac wounds.

The author of this book had under observation 136 patients with penetrating wounds of the heart, pericardium, and mediastinum. Surgical removal of foreign bodies was undertaken in 70 of these cases (28 wounds of the heart and pericardium and 42 wounds of the mediastinum). Only one of the operated patients died. A total of 73 patients (43 operated cases and 30 unoperated cases) were subjected to follow-up studies for several years.

On the basis of these studies, Grigor'ev concludes that surgical removal of the foreign bodies is a desirable procedure in most patients, with the exception of those having numerous small, well-encapsulated foreign particles that produce neither subjective nor objective disturbances. The operated patients showed more complete recovery, most of them returning to work. On the other hand, the patients without surgical intervention showed numerous disturbances that eventually led to invalidism. The author considers many of these complications to be due to disturbances in cerebro-visceral relations initiated by nerve impulses from the site containing the foreign body.

Detailed descriptions are given regarding the methods of preparing the patients for surgery, types of anesthesia used, the incision techniques, types of sutures, methods of localizing the foreign bodies, indications for surgical intervention, and the course of postoperative events. Several case-histories are included. An extensive bibliography of Russian and foreign literature is appended.

All the operations were performed under local anesthesia (0.25 percent Novocain), combined in some cases with vago-sympathetic blockade. The required manipulations of the myocardium can be performed with impunity, provided that the heart is not displaced from its normal position.

The book received the N. N. Burdenko award, one of the highest forms of recognition in the field of surgery in the U.S.S.R.

SAMUEL A. CORSON
Department of Physiology and Pharmacology, University of Arkansas School of Medicine

The Psychology of Occupations. Anne Roe. Wiley, New York; Chapman and Hall, London, 1956. 340 pp. $6.75.

This book is important because it marks the clear emergence of an idea that has never carried as much weight as it deserved in psychology as a whole—namely, that the attitudes and satisfactions related to a person's occupation are a part of the central core of his personality. This particular cross-section of what is to be found in the journals (some of them dusty and neglected of late) thus represents some important convergences. Counseling and clinical psychology meet in these pages. Social psychology, cultural anthropology, and reports on methods of personnel selection all make contributions.

The personality theory in terms of which this heterogeneous material has been approached is that of Maslow. Briefly, it states that a person has needs which are organized into a hierarchy of prepotency. The lower ones are the most imperious. As they are satisfied, those of the next higher set make themselves felt. At the top of the pyramid is the need for self-actualization. A person's occupation may satisfy many of these varieties of need at all levels of the hierarchy. A job is the accepted way of insuring freedom from hunger, and it carries with it some satisfaction of the needs for safety,

belongingness, and self-esteem. To varying extents, particular occupations also satisfy the higher needs for information, understanding, beauty, and self-actualization.

Part I, "The roles of occupations," presents these basic ideas clearly and forcefully. Part II, "People differ," is a brief but adequate summary of the field of individual differences. Part III, "Occupations differ," is the section that I found least satisfactory. The author has combed the literature for facts about the psychological characteristics of the main varieties of occupations, but the bits of information about group differences on specific tests and about correlations between personality measurements and various occupational criteria do not seem to add up to anything significant. It would appear that the research findings needed to fill out a book like this simply do not exist. I am doubtful, too, whether the particular classification of occupations the author has developed, apparently on an armchair basis, will serve its purpose, although the idea of classifying both by focus and by level of activity is a good one. For the categorization itself, however, I should prefer to trust trained occupational analysts who know what workers in the various jobs actually do. Part IV, "Following an occupation," is made up of material more closely related to the general purpose of the book, and Part V, a single chapter on "Implications," carries the ideas over into education, therapy, and psychological theory.

The book constitutes a real contribution to our thinking in psychology. Even the section that was not very satisfactory could be called "a good try." It is to be hoped that this book will stimulate the research that will eventually fill in its gaps and repair its deficiencies.

LEONA E. TYLER

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A Scientific Sampler. Raymond Stevens, Howard F. Hamacher, and Alan A. Smith, Eds. Van Nostrand, Princeton, N.J., 1956. 413 pp. $6.

This book is a collection of reports, predictions and reflections from *The Industrial Bulletin*, the magazine published by Arthur D. Little, Inc. Since the *Bulletin* covers the entire field of applied science, it follows that some of the papers record accomplishments in research and engineering, while others point the way toward developments yet to come. Topics covered range from smog to space travel. In a sense, this compilation is a chronology of science, since each article is dated. Footnotes bring the statistics up to date.

Proceedings of the Third Berkeley Symposium on Mathematical Statistics and Probability. Held at the Statistical Laboratory, University of California, December 1954; July and August 1955. vol. V, *Contributions to Econometrics, Industrial Research and Psychometry.* Jerzy Neyman, Ed. University of California Press, Berkeley, 1956. 179 pp. Illus. $5.75.

The volumes containing the two preceding Berkeley Symposia on Mathematical Statistics and Probability immediately became important works of reference for theoretical statisticians, and the group of five volumes that cover the third symposium is certain to do likewise. Nevertheless, and doubtless the organizers of these symposia have wrestled at length with this question, one wonders whether this mode of publication is the most effective one for the dissemination of this information in the quarters where it will do the most good.

This, the fifth volume of the third symposium, contains papers relating to econometrics, industrial research, and psychometry. The first paper (20 pages), by Arrow and Hurwicz, is solidly mathematical and deals with saddlepoint problems. The topic of the three following papers, by Barankin (31 pages), West Churchman (7 pages), and Suppes (13 pages), may be inferred from the fact that they all list in their references Chernoff's paper on "Rational selection of decision functions," Marschak's paper on "Rational behavior, uncertain prospects, and measurable utility," and Savage's book *Foundations of Statistics*. Undoubtedly, reactions to this material will vary somewhat.

The industrial research section starts with a review of continuous sampling plans for go-not go observations (Bowker, 11 pages). A review of fractional replication of the 2^n experiment for large n by Daniel (12 pages) contains an interesting suggestion for significance testing of a large number of contrasts. The last paper in this section, by Sobel (12 pages), discusses sequential estimation procedures for the "delayed" exponential

$$f(x;\ A,\ \mu) = \frac{1}{\mu}\, e^{-(x-A)/\mu},\quad x \geqq A.$$

The section on psychometry contains a very long and elaborate paper by Anderson and Rubin (40 pages) in which they attempt the task of reviewing factor analysis from the point of view of the mathematical statistician and contribute some new results. Mosteller (17 pages) discusses stochastic learning models, and finally Solomon (15 pages) reviews item analysis and classification techniques.

The range of topics in this volume is very wide, and a reader would be a man of great breadth of

interest to be personally concerned with more than a fraction of the volume. For example, the industrial statistician, grateful to Bowker, Daniel, and Sobel for their able treatment of real and immediate problems, will be totally uninterested in and bored by the philosophic contemplations of Barankin, Churchman, and Suppes; and though he will be filled with awe at the complexity of Anderson and Rubin's study of the mathematical basis of factor analysis, which will doubtless be invaluable to the serious psychometrician, he probably can never conceive of making use of it, and vice versa.

Certainly a large majority of the contributions are absolutely first-class, profoundly stimulating, and very rewarding of study; furthermore, some are essential and important parts of the literature to which they belong.

K. A. BROWNLEE

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Varieties of Human Value. Charles Morris. University of Chicago Press, Chicago, Ill., 1956, xv + 209 pp. Illus. $5.

As the frontier of science has changed, questions once considered beyond the reach of positive methods, have, along with empirically derived answers to them, later been incorporated into a scientific discipline. The Cartesian *res cogitans* until a century ago was, because of its nonextended character, regarded as inaccessible to measurement and beyond the realm of scientific inquiry. Not until such men as Fechner and Ebbinghaus demonstrated that at least some aspects of mind—sensation and memory—were amenable to measurement and approachable by experiment the same as extended substance is, were there widespread attempts to develop a science of mind.

Philosophers and social scientists, however, still debate the applicability of methods of science to certain aspects of human behavior, especially in the area of valuation. Charles Morris has been one of the strong supporters of the affirmative position, and his *Varieties of Human Value* is presented as evidence that rather extensive scientific inquiry into values may profitably be made. The current volume is a continuation of the author's analysis of life-orientations begun in his *Paths of Life* and pursued in his *Open Self*. These three books, along with his *Signs, Language, and Behavior,* are vital contributions to the development of human-behavior theory.

Morris has conducted an empirical investigation of human values, unmatched in scope or in molarity of level of analysis. Because of its interdisciplinary character, this work will be of special interest to psychologists, sociologists, anthropologists, and philosophers; because of the *Zeitgeist,* it will be of general interest to all thinking men.

The instrument used by Morris for measuring values was devised first by a careful search of the humanistic literature for differing philosophies of life (Christian, Apollonian, and so forth). These were briefly summarized (five to ten statements for each) and presented to groups of students with instructions to rate on a seven-point scale and rank according to preference and to add other ways to live in the event they felt those presented were not inclusive enough. This process of modification was continued until the instrument was stabilized, 13 paths of life constituting the finished document. The final version, titled "Ways to live," was then administered to large samples of college students in the United States, Canada, Norway, China, India, and Japan—nearly 6000 in all. The primary data yield information about what Morris calls *conceived* values (conceptions of the preferable), instead of *operative* values (the preferred) or *object* values (the preferable). Supplementary data were obtained concerning age, sex, physique, parentage, religion, size of town where reared, income level, personality, temperament, character, and esthetic preferences.

The physical scientist with his highly calibrated instruments may find it difficult to appreciate the immensity of the technical problems of measurement here involved. Before rather seemingly routine statistical computations could be made, the investigator had to derive scale units or to demonstrate that the response categories (ratings from *one* to *seven*) could be treated as an interval scale. With the cooperation of Lyle V. Jones, it was determined that the ratings justified the assumption of an interval scale and, amazingly enough, that the same scale could be applied throughout all the cultures surveyed.

Factor analyses of the United States, India, and China samples gave evidence of five primary dimensions in man's evaluations. These were labeled *social restraint and self-control, enjoyment and progress in action, withdrawal and self-sufficiency, receptivity and sympathetic concern,* and *self-indulgence (or sensuous enjoyment).*

When cross-cultural comparisons were made along these dimensions, Morris discovered, despite noticeable differences in preferences for specific ways, no evidence of an "East-is-East-and-West-is-West" schism in philosophy of life. In overall pattern, for example, Japan is closer to Norway

than to China, and while China and the United States are similar on self-oriented detachment, both rejecting paths of life stressing an inward-turned life, they differ markedly in other-than-self-oriented dependence—that is, on ways emphasizing receptivity to persons and nature. Widespread generalizations from the data would, of course, be inadvisable. The sample of subjects, college students, as Morris indicates, can hardly be assumed to be representative of any given cultural group; furthermore, the time of sampling—post-World War II, a period of great change—may be a significant factor in accounting for unexpected trends.

Membership in different population-size and economic groups was found to be reflected in differential ratings of the "Ways to live." For instance in the United States sample, a tendency was observed for those reared in large cities to lay stress upon social restraint and self-control and to be more oriented to the self than those reared in smaller population centers. However, since the nature and strength of the influence of these factors varied according to the cultural context, they could furnish no universal explanation of patterned preferences.

With regard to psychological variables, Morris has demonstrated certain congruence between preferences for "Ways to live" and results of various standardized tests of temperament, character, and personality, concluding that "though psychological traits are determinants of values, they alone do not provide a sufficient basis to account for valuations" (p. 112).

Using Sheldon's somatotypes as a biological variable, Morris has presented impressive evidence that some of the value dimensions are readily recognizable at the level of the organism. Definite relationships were discovered between ectomorphy (fragility and "linearity" in body build) and detachment, between mesomorphy (predominance of muscle and bone) and dominance through action, and some evidence was found of a relationship between endomorphy (roundness and softness throughout the body) and receptivity. These relationships were furthermore demonstrated to hold in the preferences for art forms; that is, the mesomorph liked best paintings reflecting activity and dominance, and so on.

One of the final chapters in this work is devoted to experimental semantics. Penetrating analyses of "good" and "ought" were made by obtaining both appraisal ratings and preference ratings on a group of paintings, by securing ratings of the "Ways to live" in terms of how one ought to live if one is to live morally, or esthetically, or religiously, and by gathering ratings describing how the raters did in fact live. The essential reference of "ought" was found to be system-required action. For example, "ought" for the moral life seems to signify "what the social system and other persons require for their maintenance and development"; for the religious life, "what facilitates a man-cosmos transaction." Likewise applying the label "good" to an object was seen to imply the arousal of a tendency to favor and to seek out the object as offering a possible satisfaction. The specific definitions are, however, probably not so important as the methods employed in arriving at them—namely, the derivation of meanings of value terms in a reasonably objective framework.

If the question is raised, "What might be done differently if this investigation were to be repeated or continued?" the aforementioned restricted nature of the subject sampling might receive first attention; then, too, the narrow range of preference behavior sampled might be extended to include other kinds of value judgments. Finally, in the treatment of the data a more direct comprehension of interaction effects might be obtained through the use of analysis-of-variance techniques.

These limitations are, however, at this stage rather minor, since the present study is more "spearheading" than "gap-filling," and their removal would probably necessitate no change in Morris's conclusion that: "The varieties of human value are not mere vagaries. They have deep roots in the phases of action, in the persistent differences between persons, and in the division of work inherent in the total process of human action" (p. 198).

WILLIAM F. DUKES
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How to Make and Use a Telescope. H. Percy Wilkins and Patrick Moore. Norton, New York, 1956. 196 pp. Illus. $2.95.

This is a delightfully informal book which unfortunately fails to give the kind of specific information needed by those to whom it is addressed. It may serve to create a desire to make and use simple astronomical equipment in many who have had no previous interest, but it does not spell out in sufficient detail *how* to do this. On the other hand, it is far too elementary for the serious amateur.

The history of the telescope is covered in nine pages, followed by a chapter giving instructions for making a simple refractor with spectacle lens optics. A few good photographs would have been much more helpful than the drawings, especially

Fig. 5, which incorrectly shows a worm drive with the axis of the worm *parallel* to the axis of the worm wheel.

The chapter on reflecting telescopes may create the desire to make a mirror, but the reader will have to consult other books for most of the details he will have to know. The description of the knife-edge test is far from adequate, and Fig. 11 gives no help at all in showing how to mount the prism. The two chapters on fitting up the telescope are short on specific information, and the chapter on eyepieces has not a single drawing to help the reader visualize what is described in the text.

The chapters on using the telescope to observe the moon, planets, and stars are the best in the book. The chapters on observing the sun and photography through telescopes would be as good if they had some diagrams or photographs to show the nature of the special equipment needed. The chapters on observatories and large telescopes are also poorly illustrated.

There are some useful appendixes, but the one on "Making a simple planetarium" bypasses all details on how to put the celestial sphere on a cylindrical tin can, and so on.

The authors are fully capable of producing a better book than this, and it is too bad they did not do it.

FRANK K. EDMONDSON
National Science Foundation

Books Reviewed in SCIENCE

2 November

Atoms and People, R. E. Lapp (Harper); *Facing the Atomic Future*, E. W. Titterton (Macmillan; St. Martin's Press). Reviewed by L. H. Snyder.

Transport and Accumulation in Biological Systems, E. J. Harris (Academic Press; Butterworths). Reviewed by L. J. Mullins.

Introduction to Electronic Analogue Computers, C. A. A. Wass (McGraw-Hill; Pergamon). Reviewed by H. K. Skramstad.

Gmelins Handbuch der Anorganischen Chemie, System No. 28, *Calcium*, pt. B, sec. 1, Technology (Verlag Chemie), ed. 8. Reviewed by H. H. Sisler.

Radiation Dosimetry, G. J. Hine and G. L. Brownell, Eds. (Academic Press). Reviewed by H. Yagoda.

Enzymes: Units of Biological Structure and Function, O. H. Gaebler, Ed. (Academic Press). Reviewed by M. H. Adams.

9 November

Currents in Biochemical Research 1956, D. E. Green, Ed. (Interscience). Reviewed by E. A. Evans, Jr.

Handbuch der Physik, vol. XIV, *Low Temperature Physics*, 1, S. Flügge, Ed. (Springer). Reviewed by R. P. Hudson.

Eléments de Mécanique Quantique, Ph. Pluvinage (Masson). Reviewed by H. Feshbäch.

Faster, Faster, W. J. Eckert and R. Jones (McGraw-Hill). Reviewed by W. W. Youden.

Cryptococcosis, M. L. Littman and L. E. Zimmerman (Grune & Stratton). Reviewed by S. B. Salvin and M. Silva.

16 November

Gaseous Nebulae, vol. 3, L. H. Aller (Wiley). Reviewed by D. ter Haar.

Encouraging Scientific Talent, C. C. Cole, Jr. (College Entrance Examination Board, New York). Reviewed by W. C. Michels.

Child Development and Personality, P. H. Mussen and J. J. Conger (Harper). Reviewed by P. S. Sears.

The American Arbacia and Other Sea Urchins, E. B. Harvey (Princeton University Press). Reviewed by A. R. Moore.

23 November

Man in Search of His Ancestors, A. Senet (McGraw-Hill); *In Search of Adam*, H. Wendt (Houghton Mifflin). Reviewed by L. Eiseley.

Handbook of South American Geology, W. F. Jenks, Ed. (Geological Society of America). Reviewed by K. B. Krauskopf.

Handbuch der Physik, vol. XIX, *Electrical Conductivity*, 1, S. Flügge, Ed. (Springer-Verlag). Reviewed by W. J. Hamer.

Psychology of Industrial Conflict, R. Stagner (Wiley; Chapman & Hall). Reviewed by W. G. Caples.

Fire Research, 1955 (H. M. Stationery Office).

Behavior Theory and Conditioning, K. W. Spence (Yale University Press; Oxford University Press). Reviewed by E. R. Hilgard.

The Chemistry of the Coordination Compounds, J. C. Bailar, Jr., Ed. (Reinhold; Chapman & Hall). Reviewed by N. H. Nachtrieb.

Agricultural Ecology, G. Azzi (Constable). Reviewed by L. W. R. Jackson.

30 November

The History of the British Flora, H. Godwin (Cambridge University Press). Reviewed by R. W. Holm.

Color Atlas of Oral Pathology, U.S. Naval Dental School, National Naval Medical Center (Lippincott). Reviewed by H. M. Fullmer.

Proceedings of the International Conference on the Peaceful Uses of Atomic Energy, vol. 8, *Production Technology of the Materials Used for Nuclear Energy* (United Nations). Reviewed by M. Kolodney.

Principles of Embryology, C. H. Waddington (Macmillan). Reviewed by D. Rudnick.

World Sea Fisheries, R. Morgan (Pitman). Reviewed by J. E. Bardach.

Jurassic Geology of the World, W. J. Arkell (Hafner). Reviewed by E. W. Berry.

New Books

Fundamentals of Chemistry and Applications. Chosen from inorganic, organic and biochemistry, with applications in physiology, microbiology, nutrition, and everyday concerns. Charlotte A. Francis and Edna C. Morse. Macmillan, New York, ed. 4, 1956. 543 pp. $6.

Margarine and Other Food Fats. Their history, production and use. M. K. Schwitzer. Interscience, New York, 1956. 385 pp. $7.

Foundations of the Theory of Probability. A. N. Kolmogorov. Translation edited by Nathan Morrison. Chelsea, New York, English ed. 2, 1956. 84 pp. $2.50.

Body Measurements and Human Nutrition. Josef Brožek, Ed. Wayne University Press, Detroit, 1956. 167 pp. $3.50.

Legg-Calvé-Pethes Syndrome and Related Osteochondroses of Youth. Charles W. Goff in association with Ned M. Shutkin and Mverma R. Hersey. Thomas, Springfield, Ill., 1954. 332 pp. $10.75.

Aerodynamics, Propulsion, Structures and Design Practice. vol. II of *Principles of Guided Missile Design.* Van Nostrand, Princeton, N.J., 1956. 595 pp. $10.

Progress in Cosmic Ray Physics. vol. III. J. G. Wilson, Ed. North-Holland, Amsterdam; Interscience, New York, 1956. 420 pp. $10.50.

The Push-Button World. Automation today. E. M. Hugh-Jones, Ed. University of Oklahoma Press, Norman, 1956. 158 pp. $3.75.

Handbook of Histology (Formerly *Handbook of Microscopic Characteristics of Tissues and Organs*). Karl A. Stiles. Blakiston Div., McGraw-Hill, New York, ed. 4, 1956. 240 pp. $3.

Evolution: The Ages and Tomorrow. G. Murray McKinley. Ronald Press, New York, 1956. 275 pp. $4.

An Introductory Course in College Physics. Newton Henry Black and Elbert Payson Little. Macmillan, New York, ed. 4, 1956. 786 pp. $6.75.

Air Pollution Handbook. Paul L. Magill, Francis R. Holden, Charles Ackley, Eds. McGraw-Hill, New York, 1956. 720 pp. $15.

Progress in Nuclear Energy. Series II, *Reactors.* R. A. Charpie, D. J. Hughes, D. J. Littler, M. Trocheris, Eds. McGraw-Hill, New York; Pergamon, London, 1956. 492 pp. $14.

Matrix Calculus. Bodewig. North-Holland, Amsterdam; Interscience, New York, 1956. 334 pp. $7.50.

Irrigation Engineering. vol. II, *Projects, Conduits, and Structures.* Ivan E. Houk. Wiley, New York; Chapman & Hall, London, 1956. 531 pp. $14.

Robert Hooke. Margaret 'Espinasse. University of California Press, Berkeley, 1956. 192 pp. $3.75.

Human Generation, Conclusions of Burdach, Dollinger and von Baer. Arthur W. Meyer. Stanford University Press, Stanford, Calif.; Geoffrey Cumberlege, Oxford University Press, London, 1956. 143 pp. $3.50.

American Social Patterns. Studies of race relations, popular heroes, voting, union democracy, and government bureaucracy. Selected and edited by William Petersen. Doubleday, Garden City, N.Y., 1956. 263 pp. $0.95.

Antarctica in the International Geophysical Year. Based on a symposium on the Antarctic. Geophysical Monogr. No. 1. American Geophysical Union, Washington, D.C., 1956. 133 pp.

Handbuch der Physik. vol. XXXVI, *Atome II.* S. Flügge, Ed. Springer, Berlin, 1956. 424 pp. DM. 88

Information Theory. Third London symposium, 1955. Colin Cherry, Ed. Academic Press, New York; Butterworths, London, 1956. 401 pp. $11.50.

Exploration for Nuclear Raw Materials. Robert D. Nininger. Van Nostrand, Princeton, N.J., 1956. 293 pp. $7.50.

Chromosomes, Sex-Cells and Evolution in a Mammal. Based mainly on studies of the reproductive glands of the gerbil and a new list of chromosome numbers of mammals. Phillip V. Tobias. Percy Lund, Humphries, London, 1956. 420 pp. 60s.

Laboratory Experiments in General Chemistry. G. Brooks King and William E. Caldwell. American Book, New York, 1956. 324 pp. $3.25.

Aquatic Insects of California. With keys to North American genera and California species. Robert L. Usinger. University of California Press, Berkeley, 1956. 508 pp. $10.

Coal-Mining. I. C. F. Statham. Philosophical Library, New York, 1956. 564 pp. $15.

Elements of X-Ray Diffraction. B. D. Cullity. Addison-Wesley, Reading, Mass., 1956. 514 pp. $10.

Engineering as a Career. Ralph J. Smith. McGraw-Hill, New York, 1956. 365 pp. $4.75.

Living Magic. The realities underlying the psychical practices and beliefs of Australian aborigines. Ronald Rose. Rand McNally, New York, 1956. $3.75.

Steric Effects in Organic Chemistry. Melvin S. Newman. Wiley, New York; Chapman & Hall, London, 1956. 710 pp. $12.50.

Process Chemistry. F. R. Bruce, J. M. Fletcher, H. H. Hyman, J. J. Katz. McGraw-Hill, New York; Pergamon, London, 1956. 407 pp. $12.

Family Medical Costs and Voluntary Health Insurance: a Nationwide Survey. Odie W. Anderson with Jacob J. Feldman. Blakiston Div., McGraw-Hill, New York, 1956. 251 pp. $6.50.

Dictionary of Photography. Reference book for amateur and professional photographers. A. L. M. Sowerby, Ed. Iliffe, London; Philosophical Library, New York, ed. 18, 1956. 719 pp. $10.

Learning and Instinct in Animals. W. H. Thorpe. Harvard University Press, Cambridge, Mass., 1956. 493 pp. $10.

Theory and Dynamics of Grassland Agriculture. Jack R. Harlan. Van Nostrand, Princeton, N. J., 1956. 281 pp. $6.75.

Seamanship. T. F. Wickham. Philosophical Library, New York, 1956. 192 pp. $3.75.

Scientific Serials. Characteristics and lists of most cited publications in mathematics, physics, chemistry, geology, physiology, botany, zoology, and entomology. ACRL Monogr. No. 16. Charles Harvey Brown. Association of College and Reference Libraries, Chicago, 1956. 189 pp. $4.25.

Atoms and People. Ralph E. Lapp. Harper, New York, 1956. 304 pp. $4.

Neurotic Interaction in Marriage. Victor W. Eisenstein, Ed. Basic Books, New York, 1956. 352 pp. $5.50.

Population Theory and Policy. Selected readings. Joseph J. Spengler and Otis D. Duncan. Free Press, Glencoe, Ill., 1956. 522 pp. $7.50.

Currents, Fields, and Particles. Francis Bitter. Technology Press, Cambridge, Mass.; Wiley, New York; Chapman & Hall, London, 1956. 599 pp. $8.50.

(*Continued on page viii*)

ASSOCIATION AFFAIRS

Pacific Division Meets

The 37th annual meeting of the Pacific Division, American Association for the Advancement of Science, was held at the University of Washington, Seattle, 11–16 June 1956. Twenty-three societies participated in the meeting, at which 500 scientific papers were presented.

Arrangements for the meeting were efficiently handled by a local committee and various subcommittees, under the general chairmanship of E. C. Lingafelter (department of chemistry, University of Washington). Registration and information headquarters were located in the Student Union Building, which also housed an interesting and instructive collection of commercial exhibits.

The committees on arrangements made it possible for the guests to enjoy to full advantage the remarkable scenic attractions that Seattle has to offer. They must have gone so far as to conspire with the Weather Bureau. And even the salmon were biting in Puget Sound. Tours were arranged to the Snoqualmie Falls Forest Camp, to Paradise Valley in Mount Rainier National Park, and to the Friday Harbor Laboratories of the University of Washington in the San Juan Islands. Special excursions were also planned so that interested persons could visit the San Juan Fishing and Packing Company of Seattle and the Boeing Airplane Company's Plant II.

A number of social events were held, including a general reception by President Henry Schmitz of the University of Washington and Mrs. Schmitz, two evening social hours, a ladies' tea, and a salmon barbecue dinner at Seward Park on the shore of Lake Washington.

There were three general evening sessions. On Monday evening, 11 June, Walter G. Whitman (Massachusetts Institute of Technology), president of the American Institute of Chemical Engineers, spoke on "The significance of the Geneva atoms-for-peace conference." On Tuesday evening, the address by Robert B. Brode (University of California, Berkeley), president of the Pacific Division, AAAS, dealt with "The boundaries of science." The concluding address on Wednesday evening was by N. Tinbergen (Oxford University, Walker-Ames professor of zoology, University of Washington), who spoke on the "Experiments on adaptive coloration in animals."

At the meeting of the council on Wednesday afternoon, Ian Campbell (California Institute of Technology) was named president-elect of the Pacific Division. The president of the division for the coming year is J. Murray Luck (Stanford University). Elected to membership on the executive committee were Walter P. Cottam (department of botany, University of Utah) and George A. Bartholomew (department of zoology, University of California, Los Angeles). Elected to the council were J. G. Hooley (department of chemistry, University of British Columbia) and Theodore L. Jahn (department of zoology, University of California, Los Angeles).

Representing the national administration of the AAAS at the meeting were Thomas Park of the University of Chicago, who came as a representative of the president of the AAAS, Paul B. Sears, and Raymond L. Taylor, associate administrative secretary.

Societies meeting in conjunction with the Pacific Division were American Chemical Society (Pacific Northwest Regional Meeting), American Institute

Table 1. Geographic distribution of registrants*

Arizona	4	Alaska	2
California	*307*	Australia	2
Colorado	3	Austria	1
Connecticut	2	Canada	
District of Columbia	4	Alberta	1
Idaho	*12*	*British Columbia*	*83*
Illinois	6	Quebec	1
Maryland	2	Saskatchewan	2
Michigan	1	Egypt	1
Missouri	2	England	2
Montana	*12*	*Hawaii*	*8*
Nebraska	5	India	5
Nevada	*0*	Iran	1
New Jersey	2	Israel	1
New Mexico	1	Jordan	1
New York	8	Netherlands	2
North Carolina	1	Norway	1
Ohio	1	Spain	1
Oklahoma	1	Thailand	1
Oregon	*155*		
Rhode Island	1	Total, territorial and foreign	121
Tennessee	1		
Texas	1	Grand total	1362
Utah	*39*		
Virginia	3		
Washington†	*664*		
Wisconsin	2		
Wyoming	1		
Total, continental United States	1241		

* Italics indicate the seven states, the territory of Hawaii, and the Canadian province of British Columbia that comprise the area of the Pacific Division of the AAAS. Their combined registration was 1,285 or 94% of the total.

† There were 34 communities in Washington represented, including 460 from Seattle and 106 from Pullman.

of Chemical Engineers (Washington-Oregon Section), American Meteorological Society (Puget Sound Branch), American Nature Study Society (Western Division), American Phytopathological Society (Pacific Division), American Society for Horticultural Science (Western Region), American Society of Ichthyologists and Herpetologists (Western Division), American Society of Limnology and Oceanography (Pacific Section), American Society of Plant Physiologists (Western Section), Association of Pacific Coast Geographers, Biometric Society (Western North American Region), Botanical Society of America (Pacific Section), Cooper Ornithological Society (Northern Division), Ecological Society of America (Western Section), Herpetologists League, National Association of Biology Teachers, Pacific Northwest Bird and Mammal Society, Pacific Slope Biochemical Conference, Society for Experimental Biology and Medicine (Pacific Coast Section), Society of Systematic Zoology (Pacific Section), Western Bird-Banding Association, Western Society of Naturalists, Western Society of Soil Science.

The 1362 registered members and guests attending the meeting were drawn from a wide geographic area, as is shown in Table 1. Included in the total registration and breakdown by states are 402 chemists and chemical engineers, most of whom preregistered separately. Several other societies maintained individual registrations and, although many of their members also registered with the AAAS, it is possible that the aggregate nonduplicate registrations in Seattle may have totaled 1550—making this one of the largest meetings of the Pacific Division.

The next annual meeting of the Pacific Division will be held at Stanford University, 25–29 August 1957, in connection with the national meeting of the American Institute of Biological Sciences to be held at Stanford at that time.

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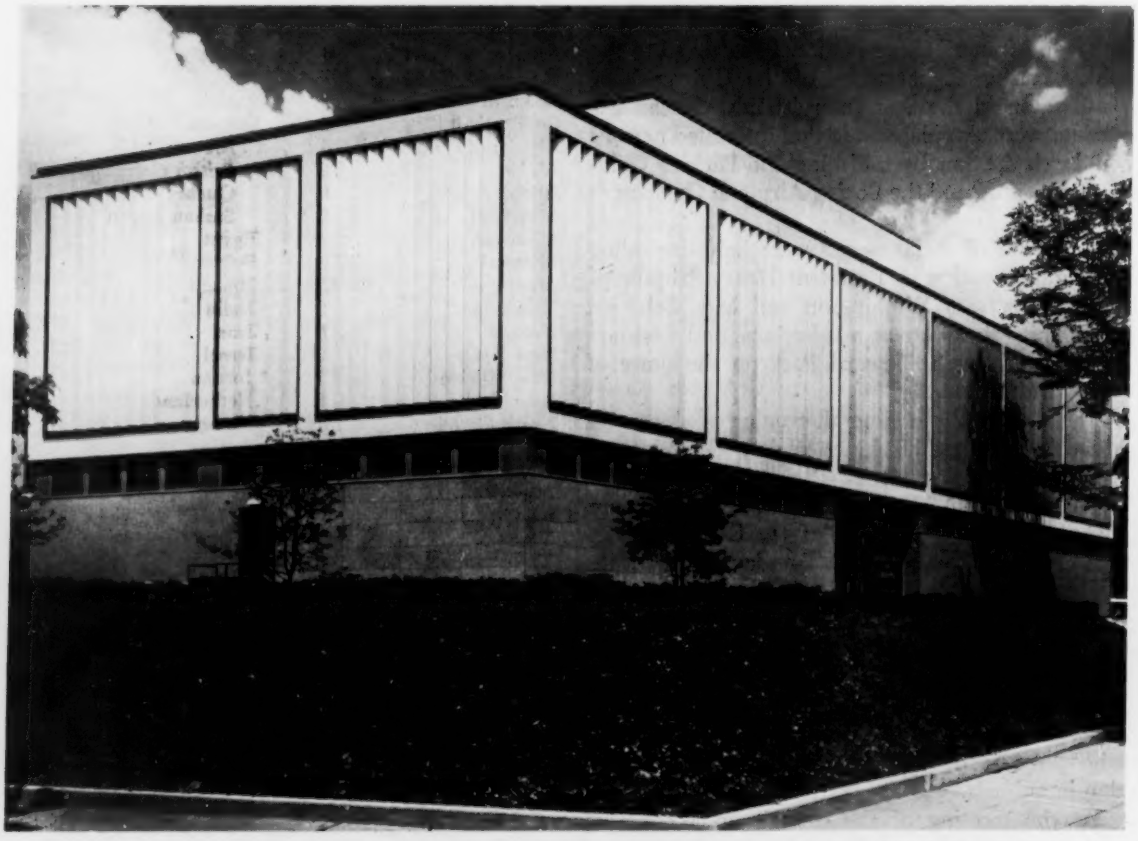

A view of the south (Massachusetts Avenue) and west (Scott Circle) sides of the new AAAS headquarters building in Washington, D.C.

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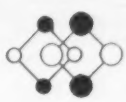

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Meetings

January

7–11. International Social Science Council, 3rd general assembly, Paris, France. (Secretary General, ISSC, 19, avenue Kleber, Paris 16.)

10. Technical and Clinical Applications of Radioisotopes, Assoc. of Vitamin Chemists, Chicago, Ill. (M. Freed, Dawe's Laboratories, Inc., Chicago 32.)

10–12. American Group Psychotherapy Assoc., 14th annual, New York, N. Y. (C. Beukenkamp, Jr., AGPA, Room 300, 345 E. 46 St., New York 17.)

14–16. Cottonseed Processing as related to the Nutritive Value of the Meal, 4th conf., New Orleans, La. (Southern Regional Research Lab., USDA, 1100 Robert E. Lee Blvd., New Orleans 19.)

14–16. Reliability and Quality Control in Electronics, 3rd natl. symp., Washington, D.C. (C. M. Ryerson, Radio Corp. of America, Bldg. 10–6, Camden 2, N.J.)

14–16. Society of Automotive Engineers, annual, Detroit, Mich. (Meetings Div., SAE, 29 W. 39 St., New York 18.)

14–20. Indian Science Cong. Assoc., 44th meeting, Calcutta, India. (General Secretary, ISCA, 1 Park St., Calcutta 16.)

15. Society for Applied Spectroscopy, Philadelphia, Pa. (F. M. Biffen, Johns-Manville Research Center, Manville, N.J.)

16–23. Australian and New Zealand Assoc. for the Advancement of Science, 32nd meeting, Dunedin, N.Z. (J. R. A. McMillan, ANZAAS, Science House, 157 Gloucester St., Sydney, N.S.W., Australia.)

17. Constructive Medicine in Aging: Cardiovascular Disorders in the Aged, symp., Cincinnati, Ohio. (J. B. Chewning, Wm. S. Merrell Co., Cincinnati.)

17–18. Engineers Joint Council, New York, N.Y. (EJC, 29 W. 39 St., New York 18.)

18–19. Symposium on Blood, 6th annual, Detroit, Mich. (W. H. Seegers, Wayne State Univ. College of Medicine, Detroit 7.)

21–22. Solar Furnace Design and Operation, Phoenix, Ariz. (J. I. Yellott, Assoc. for Applied Solar Energy, 3424 N. Central Ave., Phoenix.)

21–25. American Inst. of Electrical Engineers, winter general, New York, N.Y. (N. S. Hibshman, AIEE, 33 W. 39 St., New York 18.)

23–25. Very Low Frequency Electromagnetic Waves, symp., Boulder, Colo. (J. R. Wait, National Bureau of Standards, Boulder.)

24–25. Western Spectroscopy Assoc., 4th annual, Los Angeles, Calif. (S. S. Ballard, Scripps Inst. of Oceanography, San Diego 52, Calif.)

25. Bibliographical Soc. of America, New York, N.Y. (H. W. Liebert, Yale Univ. Library, New Haven, Conn.)

25–26. Protein Metabolism, 13th annual conf., New Brunswick, N.J. (W. H. Cole, Rutgers Univ., New Brunswick, N.J.)

28–29. Many Body Problem, symp., Hoboken, N.J. (G. J. Yevick, Dept. of Physics, Stevens Inst. of Technology, Hoboken.)

28–31. American Meterological Soc., New York, N. Y. (K. C. Spengler, AMS, 3 Joy St., Boston 8, Mass.)

28–31. Modern Methods of Analytical Chemistry, 10th annual symp., Baton Rouge, La. (P. W. West, Louisiana State Univ., Baton Rouge.)

30–31. College-Industry Conf., 9th annual, American Soc. for Engineering Education, Los Angeles, Calif. Univ. of California Extension, Engineering, Los Angeles 24.)

30–1. American Assoc. of Physics Teachers, New York, N.Y. (F. Verbrugge, Carleton College, Northfield, Minn.)

30–2. American Physical Soc., annual, New York, N.Y. (K. K. Darrow, APS, Columbia Univ., New York 27.)

31–1. Digital Computing in the Aircraft Industry, NYU–IBM symposium, New York, N.Y. (M. Woodbury, New York Univ., Research Div., 401 W. 205 St., New York.)

31–2. Western Soc. for Clinical Research, 10th annual, Carmel-by-the-Sea, Calif. (A. J. Seaman, WSCR, Univ. of Oregon Medical School, Portland 1.)

February

3. American Assoc. of Bioanalysts, 3rd annual Margaret Beattie Lecture, San Francisco, Calif. (W. N. Reich, Walnut Creek-Lafayette Labs., 1625 Locust St., Walnut Creek, Calif.)

4–8. American Soc. for Testing Materials, Philadelphia, Pa. (R. J. Painter, ASTM, 1916 Race St., Philadelphia 3.)

10–12. Canadian Ceramic Soc., 55th annual, Niagara Falls, Ont. (L. C. Keith, 49 Turner Rd., Toronto, Ont.)

14. Present Status of Heart Sound Production and Recording, symp., Buffalo, N.Y. (R. M. Kohn, Univ. of Buffalo, 2183 Main Street, Buffalo 14.)

14. Significance of Nucleic Acid Derivatives in Nutrition, Assoc. of Vitamin Chemists, Chicago, Ill. (M. Freed, Dawe's Laboratories, Inc., 4800 S. Richmond St., Chicago 32.)

14–15. Transistor Circuits, conf., Philadelphia, Pa. (G. H. Royer, Westinghouse Electric Corp., 356 Collins Ave., Pittsburgh 6, Pa.)

15–16. National Soc. of Professional Engineers, Charleston, S.C. (P. H. Robbins, 2029 K St., NW, Washington 6.)

15–17. National Assoc. for Research in Science Teaching, annual, Atlantic City, N.J. (C. M. Pruitt, Univ. of Tampa, Tampa, Fla.)

18–20. American Educational Research Assoc., annual, Atlantic City, N.J. (F. W. Hubbard, AERA, 1201 16 St., NW, Washington 6.)

18–22. American Soc. of Civil Engineers, Jackson, Miss. (W. H. Wisely, ASCE, 33 W. 39 St., New York 18.)

18–22. Endocrinology: Hormones in Blood, Ciba Foundation Colloquium (by invitation), London, England. (G. E. W. Wolstenholme, 41 Portland Pl., London, W.1.)

21–23. National Soc. of College Teachers of Education, annual, Chicago, Ill. (C. A. Eggertsen, School of Education, Univ. of Michigan, Ann Arbor.)

23. Oregon Acad. of Science, annual, Monmouth. (F. A. Gilfillan, Oregon State College, Corvallis.)

24–28. American Inst. of Mining, Metallurgical and Petroleum Engineers, annual, New Orleans, La. (E. O. Kirkendall, AIME, 29 W. 39 St., New York 18.)

24–28. International College of Surgeons, 10th biennial cong., Mexico, D.F., Mexico. (M. Thorek, ICS, 850 W. Irving Park Rd., Chicago 13, Ill.)

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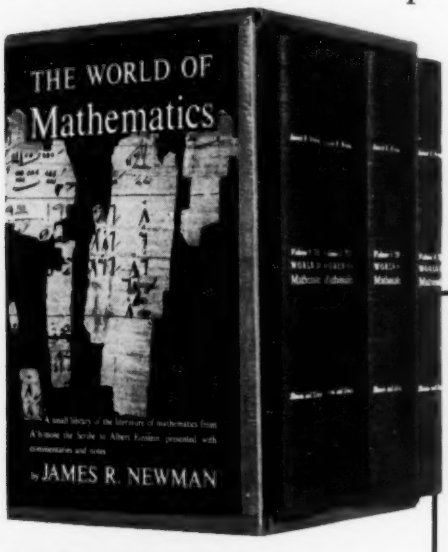

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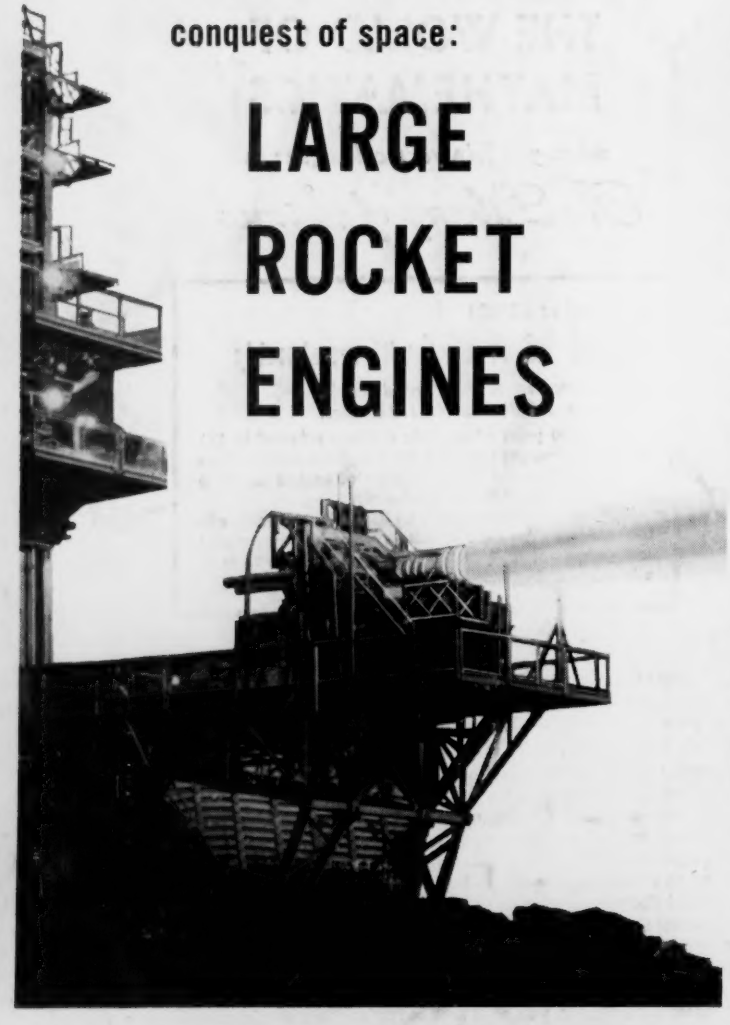

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